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Paying Disadvantaged Teenagers to Stay in School

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Abstract

We evaluate the Education Maintenance Allowance, a large conditional cash transfer scheme that paid low-income teenagers in England to remain in education beyond age 16. Using the staggered national roll-out of the programme and linked administrative data tracking education, earnings, welfare payments and criminal convictions to age 31, we find no significant overall effect of the policy on labour market outcomes or criminality. High-attaining students were more likely to attend university but no more likely to graduate. Low-attaining students committed fewer crimes. We estimate the Marginal Value of Public Funds was 0.85 (95% confidence interval 0.52–1.29); even at the upper bound of this interval, benefits barely outweigh costs.

JEL classification

I28, J24, H52

Keywords

conditional cash transfers, education, crime

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Children born into poverty face formidable barriers to economic success. In the UK, among those born in the mid-1980s, the 15% of students poor enough to qualify for free school meals were 40% more likely to leave formal education at age 16, over twice as likely to be convicted of a crime, and 30% more likely to earn below median wage in adulthood. Improving disadvantaged students' upward social mobility through increased educational attainment was a key priority for the New Labour government that took office in 1997.[1]

A flagship policy was the Education Maintenance Allowance (EMA), which we study in this paper. First introduced in a pilot in 1999 and 2000 and rolled-out nationally in 2004, the EMA provided payments of up to £30 per week, or up to £1,400 per year (US$70 and US$3,300, respectively, in 2023/24 prices) to 16–19-year-olds from lower-income households who remained in full-time education beyond the minimum school leaving age of 16 and regularly attended classes. This was a large programme: the full award amounted to around one-third of per-pupil funding received by schools and colleges from the government, and more than half of young people were eligible if they chose to remain in full-time education. The program was widely advertised and, in particular, paid benefits directly to students rather than parents.

Low rates of educational participation and high inactivity among 16–19-year-olds pose major challenges in many advanced economies. While raising the legal participation age is a potential way to address this, enforcement becomes more difficult as young people get older, and ensuring regular attendance is even harder. In this context, providing direct financial incentives—paying students to enrol and attend—emerges as a natural policy tool. Although conditional cash transfer (CCT) programmes have shown large benefits in low- and middle-income countries, it is unclear whether similar approaches can improve outcomes in higher-income settings, where the marginal costs and returns of education may be very different. The EMA is the largest scheme of this nature implemented in a high-income country. While short-run impacts based on a pilot phase of the EMA have been studied, there is no evidence on the short- and longer-term impacts of the national roll-out of the policy.

This paper fills the evidence gap by examining the effects of the national roll-out of the EMA on the outcomes of students from low-income households in England. We use newly linked administrative data with near-universal coverage, tracking approximately 600,000 individuals per cohort until their early thirties. The richness of the data allows us to study enrolment in education, qualifications, criminality, welfare receipt and earnings outcomes. A novel aspect of this work is our access to both long-run labour market outcomes and criminal records for the same individuals. We construct two separate but complementary administrative data linkages: one connecting education records to earnings and benefit receipt up to age 31, and the other linking education to cautions and convictions for crimes committed between ages 14 and 30, adding to the growing body of UK studies evaluating an education policy by tracking

[1] In his party conference speech in October 1996, the Labour party leader and future prime minister Tony Blair famously declared that his three main priorities for government were "education, education and education", with an explicit focus on making educational opportunities available to all.

individuals into adulthood using administrative data.[2]

To estimate causal effects, we exploit the staggered roll-out of the programme by school cohort across areas in England. We use difference-in-differences comparisons across cohorts between areas that received the policy before 2004 and those that received it in 2004 (the linked administrative data do not go back far enough for us to evaluate the pilot phase of the programme). To guard against differential trends in outcomes, our main results concentrate on a subset of treatment and control areas that were observably similar before the national roll-out. We focus on the 15% of students who were eligible for free school meals at age 16 in these areas (around 25,000 students per cohort), virtually all of whom qualified for the full EMA award.

We find that despite high take-up, the EMA did not improve the labour market outcomes of eligible recipients up to age 31. This null result is consistent with our finding that the programme had only a modest impact on education participation (an increase of around 3 percentage points on a baseline of 51%) and that it had almost no overall impact on qualifications. The precision of our estimates allows us to rule out earnings gains larger than 0.3% per year through age 31, conditional on employment.

The result broadly holds when we split the analysis by prior attainment based on test scores at the end of compulsory schooling. If anything, there is some evidence of *weaker* employment and earnings outcomes among young people in the tails of the prior attainment distribution. Among those with high prior attainment, the EMA reduced part-time work while studying, led to slightly better results in age-18 exams (A-Level) and increased university entry, but without significantly increasing university graduation rates. These students earned slightly less in their early 20s, likely reflecting extended time in education, but this was not compensated by higher earnings between ages 26 and 31. For those with low prior attainment, while post-compulsory participation rose in the first post-compulsory year, there was no impact on additional qualifications, and individuals were more likely to be economically inactive at age 18. They engaged less in work-based training, had lower earnings throughout their 20s, and received more welfare benefits.

Conversely, we do observe a reduction in criminal convictions during and after the period of EMA eligibility for those with low prior attainment. Nonetheless, our central estimate for the Marginal Value of Public Funds (MVPF) is only 0.85 (95% confidence interval: 0.52-1.29). Since this is a *transfer*, an MVPF of one represents a natural benchmark; our estimates therefore rule out the possibility that the EMA was a considerably better investment than its direct transfer value alone would suggest, and our central estimate falls below this benchmark.

The results are robust to a wide range of sensitivity checks. We experiment with changes in the control variables and samples used in estimation. Our findings are not dependent on our decision to focus on a subset of matched areas rather than the whole country. We also test the sensitivity of our findings to the choice of pre- and post-treatment cohorts. None of these alternative specifications meaningfully change the results. Our heterogeneity analysis also supports the overall conclusions: splitting results by prior attainment, gender, special

[2] Britton et al. (2022) and Aucejo et al. (2025) are other examples.

educational needs status, and ethnicity, we find no evidence of improved labour market outcomes by age 31 in any subgroup. Finally, when we approximate effects for the full population of EMA eligible students—rather than focusing only on students eligible for free school meals—we find similar effects on participation and qualifications.

Our work contributes to a broader literature on how education policy for young people in their late teens can address low participation rates. Raising the compulsory school-leaving age among older adolescents is difficult to enforce and may have limited effects (Clark, 2023; Stephens Jr and Yang, 2014; Pischke and von Wachter, 2008). Alternative approaches to raise participation such as information interventions are unlikely to substantially change enrolment decisions (McGuigan et al., 2016; Bleemer and Zafar, 2018). CCTs in lower-income countries have had positive impacts on education participation and academic performance (Barrera-Osorio et al., 2011; Attanasio et al., 2012; Glewwe and Kassouf, 2012; Galiani and McEwan, 2013), as well as on university attendance (Barrera-Osorio et al., 2019) and earnings (Duflo et al., 2023). The evidence from high income countries is limited but also encouraging: Dearden et al. (2009) found that the pilot phase of the EMA raised participation by around 6.7 percentage points.[3] Dearden and Heath (1996) found comparable participation effects of a similar programme in Australia, and Riccio et al. (2013) found small positive effects on education outcomes from the the Opportunity NYC–Family Rewards programme (which combined education incentives with other features).[4] Similar financial aid schemes targeting university students have also been effective at raising enrolment and persistence, with evidence from the US and UK indicating that grants can increase higher education participation and improve degree outcomes for disadvantaged students (Dynarski, 2003; Denning et al., 2019; Murphy and Wyness, 2023). Finally, CCTs do not just enforce enrolment, they also provide direct incentives to attend classes. Fryer Jr (2014) and Guryan et al. (2023) find that interventions promoting school engagement can improve academic performance even among students already enrolled.

Yet, our findings suggest that schemes like the EMA are unlikely to improve the longer-run economic outcomes of disadvantaged youth in high-income settings. They are consistent with evidence that early school dropout does not cause large penalties in adulthood (Andresen and Løkken, 2024) and with findings that paying students for results has not been effective (Allan and Fryer, 2011). Taken together, these results suggest that simply providing financial incentives to extend time in education, without simultaneously improving qualifications or addressing deeper barriers to engagement and achievement, is unlikely to generate meaningful economic gains. This highlights the importance of designing policies that focus not only on increasing participation but also on improving the quality and relevance of educational experiences.[5]

[3]This is roughly double the participation effect that we find from the national roll-out. We attribute this difference to methodological factors, the declining real value of EMA between the initial pilot phase and the national roll-out, and differences in how participation was measured in their survey data versus our administrative data. See Section IV A 1 for a more detailed discussion.

[4]None of these studies use administrative data, nor do they examine longer-run outcomes.

[5]While the long-run effects of job training programmes targeting disadvantaged young people have been similarly disappointing (Schochet et al., 2008), Cavaglia et al. (2020) has suggested that apprenticeships may offer substantial

Our work also contributes to understanding the relationship between education policy and crime. If programmes like the EMA succeed in incentivising education participation, they might reduce idle time when pupils could potentially commit crimes. (Jacob and Lefgren, 2003; Machin and Meghir, 2004). Bell et al. (2022) show that these "incapacitation effects" can reduce crime in the short- and long-run. The additional income provided by the transfer may also reduce economic incentives for crime, as in traditional Becker (1968) models (Foley, 2011; Chioda et al., 2016; Blattman et al., 2017; Watson et al., 2020). On the other hand, higher income could increase opportunities for risky behaviours (Watson et al., 2020). Our finding that the EMA reduced crime among those with low prior attainment is consistent with an incapacitation and a wealth effect. We find additional support for the latter by providing suggestive evidence of a fall in thefts. We find no evidence of increased risky behaviours (specifically, zero impact on drug-related convictions). Our finding that the negative impacts on crime persist for the lowest prior attainment group is consistent with Bell et al. (2018), which shows that increased crime in early adulthood can have persistent long-run impacts on criminal activity years later.[6]

I Institutional background

I A Post-16 options in England in the early 2000s

In England during the early 2000s, compulsory education ended after the academic year in which students typically turn 16 ('Year 11').[7] At the end of Year 11, nearly all students took nationally standardised General Certificate of Secondary Education (GCSE) examinations, typically in around ten subjects. These exams were considered "passed" if graded from A* to G. Grade C served as a key threshold for academic progression: students who achieved at least five GCSEs at grade C or above were eligible to progress to A-Levels, the primary route to university. These two-year academic courses were offered in both secondary schools and Further Education (FE) colleges.

Alternative post-16 education options included lower-level academic courses, such as GCSE retakes, as well as vocational qualifications. From ages 16 to 18, vocational qualifications were available at Level 1 (basic), Level 2 (roughly equivalent to GCSE level), or Level 3 (roughly equivalent to A-Levels), depending on prior attainment, with Level 2 being the most common.[8] These programmes were typically delivered in FE colleges and could be taken full- or part-time. Those choosing to leave education could enter employment without training or enrol in various training schemes. Work-based training programmes (such as Apprenticeships) combined paid

positive returns in some fields for young people.

[6]Our work is also consistent with Sabates and Feinstein (2008), who used aggregated local crime data to show that that EMA pilot areas experienced small reductions in crime immediately after the introduction of the policy (at a rate of roughly of 1-1.5 convictions per 1,000 individuals).

[7]The academic year in England runs from September 1 to August 31.

[8]Examples of vocational qualifications at this time include NVQs (National Vocational Qualifications) in hairdressing or health and social care, BTECs (Business and Technology Education Council) in business or engineering, and diplomas or certificates in areas such as construction or hospitality.

employment with structured training. Unpaid preparatory schemes (such as Entry to Employment) provided training and an allowance paid by the government.

Among FSM-eligible students that turned 16 in 2003 or 2004, 59% remained in full-time education at age 16, 6% were in part-time education, 13% were in training, and 21% were not in education or training. Around 90% of the latter group were not in employment, education, or training (NEET); the rest were in employment without training. To capture variation in students' opportunities and likely trajectories, we divide individuals into three prior attainment groups for much of our analysis: high attainment (at least five GCSEs at grade C or above), medium attainment (five GCSEs at grade G or above but fewer than five at grade C or above), and low attainment (fewer than five GCSEs at grade G or above). 28% of FSM-eligible were in the high attainment group (compared to just over half of all students), while 23% were in the low attainment group (compared to around 10% of all students).[9]

I B The Education Maintenance Allowance (EMA)

The EMA provided weekly payments to 16-19-year-old students from low-income backgrounds who remained in full-time education. Most students could claim the allowance for up to two years between the first post-compulsory academic year ('Year 12', the year they turned 17), and the third ('Year 14').[10] To be eligible, students had to be above the compulsory school leaving age and enrolled in a full-time academic or vocational education course. Advanced courses (above Level 3; most notably, higher education) and training were excluded.

The payment varied with parental income (Ashworth et al., 2001). Students with parental income up to £19,630 in the preceding tax year (37% of the population) received £30 per week during term time, totaling £1,200 annually (£48 and £1,930 in 2023/24 prices).[11]Students with parental income between £19,630 and £24,030 (11% of the population) received £20 per week. Students with parental income between £24,030 and £30,000 (10% of the population) received £10 per week. Additional incentives included up to £200 in bonuses for completing each school term and for good examination performance, meaning the maximum annual award was £1,400 (£2,250 in 2023/24 prices). This was a substantial sum, equivalent to nearly a quarter of full-time minimum wage earnings for 16- and 17-year-olds or about a third of direct per-pupil funding for schools and colleges.[12] Payments were contingent on attending all lessons each week and

[9]Our classification of prior attainment also includes grades from the vocational "GNVQs", which were GCSE equivalents taken during compulsory schooling, typically alongside GCSEs.

[10]Students receiving support for Special Educational Needs could claim the allowance for up to three years.

[11]All threshold values in this section are for the 2004/05 school year. The parental earnings thresholds were increased once for the 2005/06 school year (to £20,817, £25,521 and £30,810) and then remained frozen in nominal terms until the EMA was abolished in England in 2011. The amount of weekly cash support available was frozen in nominal terms until 2011. Eligibility shares are based on reported parental earnings in the Next Steps survey of the 1989/90 birth cohort, which became eligible for the EMA in 2006/07. The shares are corrected for measurement error in parental income using a parametric model based on reported EMA receipt in Next Steps (see Section I C). The uncorrected estimates are similar at 36%, 10% and 10%, respectively, for the share of the population eligible for £30, £20 and £10.

[12]The minimum wage for 16- and 17-year-olds was introduced at £3 per hour—or £120 per 40-hour work week—shortly after the EMA was rolled out nationwide. At the same time, schools and colleges received around £4,500 and £4,000, respectively, in direct funding per 16-18-year-old pupil (Britton et al., 2020).

authorising any absences (Hubble, 2008).[13]

I C Take-up of the EMA

As no administrative data on parental income is available, we investigate take-up using survey data. The Next Steps survey of children born in England in the 1989/90 academic year is well-suited for this purpose because it asked young people about EMA receipt and whether they were in full-time education, and their parents about whether their income fell within specified ranges. Among full-time students aged 16-17 in the 2006/07 academic year, 32% reported receiving the full amount of the EMA, and 42% reported receiving any EMA.[14] This compares to 31% of parents reporting income below £20,800 (when the cut-off for the full EMA was £20,817), and 51% of parents reporting income below £31,200 (when the cut-off for any EMA was £30,810).

These figures suggest very high take-up among students eligible for the full award. Some young people reported receiving EMA amounts inconsistent with their parents' reported income, suggesting that survey-reported income differed from income as measured for EMA eligibility purposes. Reasons for this may include some parents not precisely knowing their partner's income or having a different concept of income in mind than what mattered for determining EMA eligibility. To determine take-up rates despite potential measurement error in parental income, we estimate a parametric model using the Method of Simulated Moments.[15] This suggests take-up rates of 89% among those eligible for £30, 55% among those eligible for £20, and 42% among those eligible for £10.[16]

These figures are consistent with official statistics on the share of EMA recipients among all students in full-time education at age 16 to 19 who were potentially eligible based on their home address and birth cohort. This share was roughly constant at around 40% between the 2004/05 and 2008/09 academic years (see Appendix Table A2).[17] This is roughly comparable to 42% of

[13]For more detail on the policy, see Middleton et al. (2005) and Ashworth et al. (2001). See Online Appendix Section 1 for information on other education-related welfare payments available to families with children in this age group.

[14]For comparability, these statistics are conditional on parents reporting their income a year earlier, which is true for 83% of households in the sample. Reported statistics are population analogues obtained using sampling weights.

[15]Specifically, we estimate a model where the logarithm of parental earnings as relevant for EMA eligibility is $\log(y_i^*) = \mu_a + \sigma_a \eta_i$, the logarithm of reported parental earnings is $\log(y_i) = \log(y_i^*) + \mu_m + \sigma_m \epsilon_i$, and η_i and ϵ_i are independent draws from the standard Normal distribution. The share of young people who do not take up the EMA is γ_{30} for those who are eligible for the full award, γ_{20} for those eligible for £20, and γ_{10} for those eligible for £10. The moments we use to identify these parameters are the share of households in each band of reported earnings and the shares of young people taking up each category of the EMA within each observed household earnings band. The share of households in each band of reported earnings identifies the mean $\mu_a + \mu_m$ and the variance $\sigma_a^2 + \sigma_m^2$ of reported earnings. The spread of EMA take-up across categories of reported earnings separately identifies μ_m and σ_m, and the level of EMA take-up within each observed earnings category identifies γ_{30}, γ_{20} and γ_{10}. The reported parameters minimise the distance between the sample moments and simulated moments based on $100,000$ simulated households, weighted by the share of households used to compute each sample moment.

[16]The estimates imply that earnings as relevant for EMA eligibility were on average 7 log points lower than earnings in the Next Steps survey. The estimated standard deviation of the difference was 24 log points.

[17]The slight increase in the share of potentially eligible students claiming the EMA between the 2005/06 and 2006/07 academic years is likely related to the re-classification of some forms of work-based learning as full-time education for the purposes of EMA eligibility in 2006 (see Online Appendix 1.1).

young people in full-time education in Next Steps reporting receiving the EMA at age 16.[18]

I D The staggered roll-out of the EMA and eligibility

First announced in spring 1999, the EMA was initially rolled out as a pilot scheme in 15 out of 150 English Local Authorities (LAs) in September 1999. These were predominantly deprived, urban areas outside London. Students living in these LAs were eligible for up to £30 or £40 per week, depending on their LA's pilot scheme variant and their parental income. At that time, 11 LAs were designated as control areas, chosen for their similar characteristics to the pilot LAs.[19] In September 2000, the pilots expanded to another 41 LAs; the 11 control LAs remained excluded. For the next four years, LAs containing about one-third of England's population—mostly urban areas with higher deprivation levels—had access to the EMA, while the other two-thirds—predominantly rural and suburban areas with lower deprivation—did not. In 2004, the EMA was rolled out to the rest of England (the 'national roll-out').[20]

Eligibility for the EMA was based on both where someone lived and their birth cohort. Students could participate in education in a different LA to the one they lived in but eligibility was based on their home LA, meaning they could not cross LA borders to receive the EMA. There was no partial eligibility: individuals living in areas that gained access to the EMA in the national roll-out who completed compulsory schooling before 2004 were never eligible.[21]

The EMA was abolished in England in 2011 and replaced by the less generous 16-19 Bursary Fund, which was allocated according to education providers' discretion. The EMA continues to operate, with some modifications, in Scotland, Wales and Northern Ireland.

II Data

We use several administrative datasets to estimate the effect of the EMA on educational, labour market, and criminal outcomes. For education and labour market outcomes, we use the Longitudinal Educational Outcomes (LEO) dataset, which links school records from the National Pupil Database (NPD) to administrative records on education at FE colleges and universities as well as tax and welfare benefit records. For crime, we link the NPD to criminal records in a separate environment from the LEO data.

The NPD covers all state-educated children in England starting with the 1985/86 birth cohort, which finished compulsory education in 2002 (after the start of the pilot phase of the EMA, but

[18]The figures are not precisely comparable because (a) official aggregates include full-time students at all eligible ages in a given year, and (b) some full-time students aged 18-19 will already have received the EMA for two years and therefore will have been ineligible for a third year, except if they had Special Educational Needs (SEN).

[19]Dearden et al. (2009) used 9 of the 15 treated areas and 9 of the 11 pilot control areas in their evaluation of the pilot.

[20]It was also rolled out in Scotland, Wales and Northern Ireland in 2004. It has not been possible to access data from these nations for comparison with England.

[21]While different LA variants of the EMA had existed from 1999-2004 (varying in payment amounts, income thresholds, achievement bonuses, and whether the allowance was paid to the student or the parents), a uniform version paid to students directly was rolled out across LAs in 2004. Our results are not sensitive to the inclusion of LAs that experienced small changes to their versions of the EMA in 2004.

before the national roll-out). It contains data from nationally standardised academic exams, including at age 11 (Standard Assessment Tests), 16 (typically GCSEs), and 18 (A-Levels). The NPD also contains data on whether students remained in school after age 16 and background characteristics including gender, ethnicity, first language, special educational needs status, whether a student received free school meals, and an index of neighbourhood deprivation.[22]

As the school records do not include parental income or a marker of EMA eligibility, we instead focus our analysis on individuals who received free school meals at age 15-16 ('Year 11'). Eligibility for free school meals was based on concurrent access to certain means-tested welfare benefits, so recipients were typically within the bottom 20% of the population by parental income.[23] Since eligibility for the EMA depended on family income in the previous tax year, and since 37% of the population were eligible for the full award, almost everyone who was eligible for free school meals age 15-16 would also have been eligible for the full EMA award at age 16-17.[24]

Data on participation in Further Education comes from the Individualised Learner Record (ILR), linked by pupil matching reference (PMR) to the NPD. This has details on whether a student was attending a Further Education college in each academic year. It also provides a record of all of the courses they studied, and whether students successfully completed them.[25] The ILR also enables us to distinguish between education and work-based training, such as an apprenticeship. This is an important distinction as people in work-based training were not eligible for the EMA. Data on university education comes from the Higher Education Statistics Authority (HESA)—also hard linked by PMR—from which we use information on attendance and whether a university degree was obtained.

Administrative data from HMRC, the UK tax authority, allow us to link education records to long-run earnings and welfare benefit receipt. Individuals are first matched from the NPD to national insurance records using name, gender, date of birth and home address; subsequent linking across tax and welfare benefit records is then by national insurance number. Non-matches arise either because individuals cannot be successfully linked to the national insurance register, or because they never interact with HMRC—for example, if they never report taxable earnings or claim welfare benefits during our observation window (including due to death or emigration). Appendix B shows that match rates between the NPD and HMRC records are high, at 91–95% for

[22]Specifically, it contains the Income Deprivation Affecting Children Index (IDACI) based on each student's home address. neighbourhoods are defined by Lower layer Super Output Areas (LSOAs), which comprise between 400 and 1,200 households.

[23]For some of these welfare benefits, such as Child Tax Credit, eligibility for free school meals also required recipients' household income to be below a certain threshold.

[24]Using the Family Resources Survey, we estimate that this figure is over 99%. While some of those eligible for free school meals at age 15-16 will no longer have been eligible for the EMA at age 17-18, we expect this to have been rare. In the Next Steps survey, 88% of parents of 16-17-year-olds in 2005/06 who had earned less than £15,600 the year before (an approximate threshold for free school meal eligibility) reported earning less than £20,800 that year, when the threshold for the full EMA award was £20,817. Given measurement error in the Next Steps survey, the true share of students eligible for free school meals at age 15-16 who remained eligible for the full EMA award at age 17-18 was likely even larger.

[25]We also use data from the Young Person's Matched Administrative Dataset (YPMAD), a derived dataset from the NPD and ILR which provides consolidated information on students' activities in a given school year.

our main population of interest.

HMRC reports total earnings from employment in each tax year from 2003/04 onward for all individuals who are successfully matched to the NPD, allowing us to observe students' earnings during and after leaving education. Data is not available on other aspects of employment, such as hours or occupation. We classify an individual as employed if their HMRC-recorded earnings exceed the Lower Earnings Limit (LEL; £4,108 in the 2004/05 tax year). Employers were obliged to report pay only when it exceeded the LEL, so this threshold provides a natural and conservative threshold for defining employment, consistent with the statutory reporting requirement than using annual earnings above zero.[26]

The tax data also include information from the Department of Work and Pensions (DWP) on individual welfare benefit spells. These cover whether benefits were received, for how long they were received, and whether these were out-of-work benefits, such as unemployment benefits or income support for those unable to work. This allows us to estimate the effect of the EMA on different types of welfare benefits and gives us an additional indicator of non-employment.

For the evaluation of the effect of the EMA on criminal outcomes, we use the Ministry of Justice MOJ-DfE database. This source links the NPD dataset described above with criminal records from the Police National Computer (PNC). The linkage uses identifiers such as name, date of birth and address through a combination of deterministic and probabilistic matching. Independent assessment suggests that this linkage is of reasonably high quality (ADRUK, 2022). The data include individual-level records on offences for which an individual receives a caution or conviction in court. For each offence, the PNC includes details on the type (for example, drug offences), the date it was committed, and the caution or sentence received. We focus on offences for which individuals have been convicted in court, as standards for convictions are closely aligned across the country. We observe convictions between January 2000 and December 2020.[27]

Our data cover the eight cohorts that finished compulsory schooling between 2002, who were typically born between September 1985 and August 1986, and 2009, born 1992/93 (hereafter the 2002 to 2009 cohorts). Our key educational outcomes of interest are the main activities of individuals at age 16-17 and age 17-18; qualifications other than university degrees up to 7 years after completing compulsory schooling; and university degrees up to 10 years after completing compulsory schooling. Since our last year of tax data is 2020/21, we can track labour market outcomes up to 11 years after completing compulsory schooling (age 27) for all 8 cohorts, and up to 15 years for the 4 oldest cohorts in our dataset (age 31).[28] The last academic year for which we have a full record of crimes committed is 2017/18, so we can track criminal activity in the 4 oldest

[26]There is no observable discontinuity in the earnings data at the lower earnings limit, suggesting that virtually all employers reported earnings for all their employees.

[27]To guard against biases from sentences being handed down after the end of our observation period, we only report results for crimes committed at least 28 months before the end of our observation period and disregard the 2% of crimes where there are more than 28 months between date the crime was committed and the sentencing date. Therefore, the last academic year for which we observe a full record of crimes is the 2017/18 academic year.

[28]We refer to the tax year n years after the end of compulsory schooling as 'age $16 + n$' because all individuals in our sample were age 16 on the August 31 when they finished compulsory schooling. August 31 is slightly before midway through the tax year, which runs from April 7 to April 6.

cohorts in our dataset for up to 13 years after compulsory schooling. Our main difference-in-differences estimates use the four cohorts from 2002 to 2005 (see Section III A). The full eight-cohort sample is used for event study analyses.

III Methodology

III A Main estimates

To estimate the impact of the EMA on education, labour market, and criminal outcomes, we use a difference-in-differences (DiD) approach across cohorts at the LA level. We compare cohort-by-cohort changes in outcomes across individuals residing in two groups of areas: LAs that received the EMA in 1999 or 2000 (the early roll-out areas), and LAs that received it later in 2004 (the national roll-out areas, denoted NR). In our main regressions, we include four cohorts, comparing the two cohorts which completed compulsory schooling before the national roll-out (in 2002 and 2003) with the two cohorts which entered post-compulsory education immediately after the national roll-out (in 2004 and 2005). Our baseline specification is:

$$y_{ijc} = \alpha_j + \nu_c + \beta Post_c \times NR_j + \delta X_{ijc} + \eta_{ijc} \tag{1}$$

where $\hat{\beta}$ is interpreted as the average causal effect of the EMA on the outcome of interest y_{ijc} for individuals in the 2004 and 2005 cohorts in the national roll-out areas. Local authority fixed effects, denoted by α_j, capture time-invariant attributes of each local authority. Cohort-level fixed effects, denoted by ν_c, capture cohort-level attributes that do not change across areas. We also include a set of individual and neighbourhood-level controls in vector X_{ijc}. The individual controls are gender, ethnicity, whether English is their first language, whether they have Special Educational Needs, and attainment from national tests taken at age 11 and age 16.[29] For criminal outcomes, we include observations of the same outcome from before individuals became eligible for the EMA at age 16 as additional individual controls.[30] The only neighbourhood-level control is the neighbourhood deprivation index. We compute standard errors clustering at the LA level.

Our empirical strategy differs from a traditional DiD design in that our "control" group—the early roll-out LAs—was already receiving the EMA at the beginning of our study period. In contrast to a standard DiD setup, where one group transitions from untreated to treated and the other remains untreated, in our case the other group is *always* treated.[31] Identification thus relies on the assumption that the change in outcomes observed in the always-treated areas between the two cohorts immediately before and immediately after the national roll-out provides a valid

[29] We show in Section V A that the results are not sensitive to the inclusion of the age 16 test results as controls.

[30] For example, if the outcome is an indicator for whether an individual received a theft conviction for a theft committed at age 16-17 or age 17-18, we include indicators for any theft in Year 9 (age 13-14), any theft in Year 10 (age 15-16) and an theft in Year 11 (age 15-16). Because our data on convictions only begins on 1 January 2000, we disregard crimes committed in the first 4 months of Year 9 to ensure comparability across cohorts.

[31] Empirical papers that have used similar approaches include Kim and Lee (2019), Sawada et al. (2022) and von Hinke and Sørensen (2023) (though in the latter case, one group transitions from treated to untreated, while the other is always untreated).

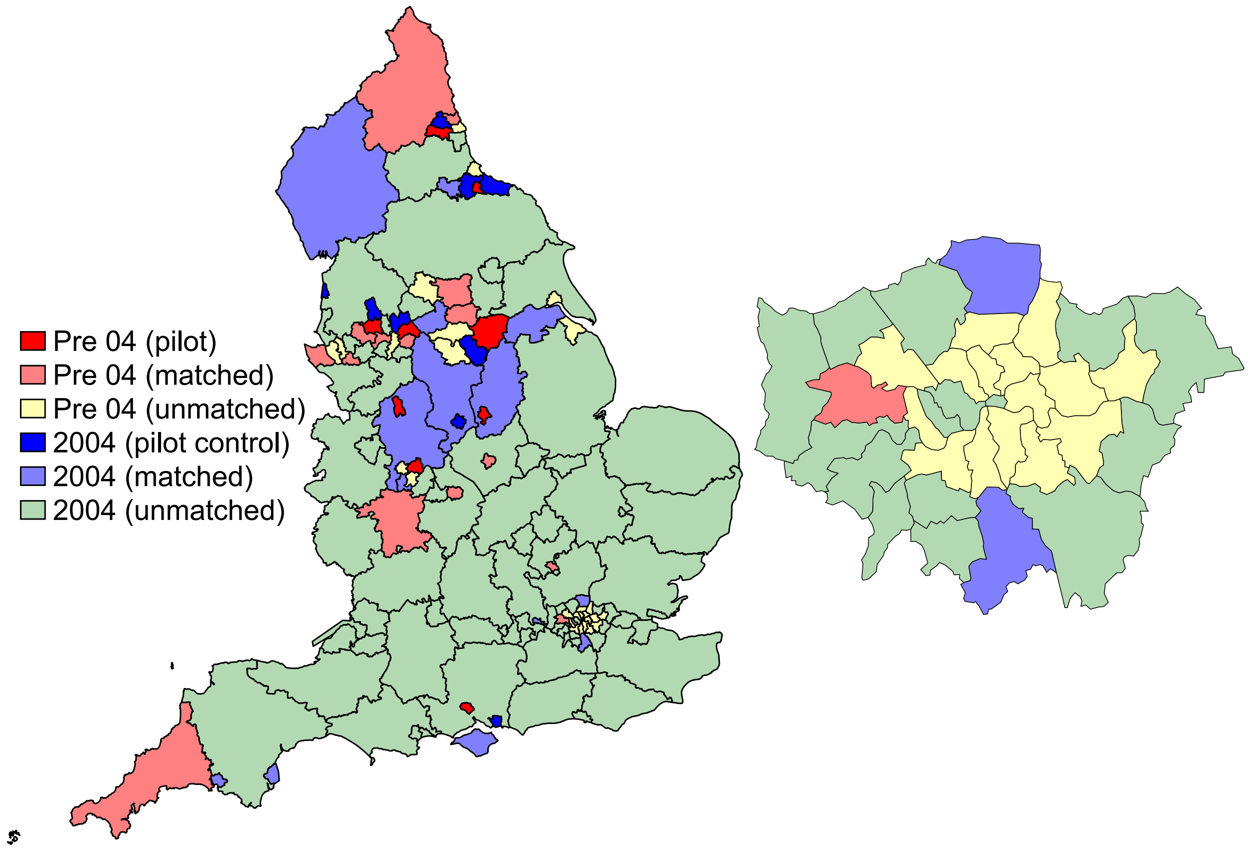
counterfactual for what would have happened in the national roll-out areas in the absence of treatment (the common trends assumption). In addition to the standard common-trends assumption, this generally requires the treatment effect in the always-treated (early roll-out) areas to be stable across the cohorts included in our analysis.[32]

This additional assumption is more plausible in our setting than in a typical DiD setting because we are comparing cohorts composed of different individuals rather than individuals at different times, ruling out most sources of dynamic treatment effects. As always-treated areas were first treated at least four years before the national roll-out, we can allow for implementation lags of up to two years in the EMA pilot areas. Education participation rates were reasonably stable during the period we are studying, making it plausible that there were no substantial changes in the share of children at the margin of staying in full-time education across cohorts. Inflation was around 2% during this period, so there were also no large changes in the value of the EMA grant. If anything, a mild amount real-value erosion would slightly attenuate the treatment effect in early roll-out areas, biasing our estimates slightly upward.[33]

[32]An alternative approach to identification is to allow treatment effects to vary across cohorts but to assume that they are constant across areas within a given cohort (which would imply no implementation lag in the national roll-out). Under this "reverse difference-in-differences" interpretation (Kim and Lee, 2019), $\hat{\beta}$ would be an estimate of the causal effect of the EMA on the 2002 and 2003 cohorts. Because the traditional difference-in-differences framework is more familiar and the interpretation more straightforward, we adopt this as our preferred strategy. The results could equivalently be interpreted through this "reverse difference-in-differences" lens.

[33]Another challenge to this assumption is that some of the early treated areas received different versions of the EMA before 2004. Our results are not sensitive to excluding these areas (see Section V A).

Figure 1: Education Maintenance Allowance Roll-out



Notes: Map of Local Authorities (LAs) in England by year of EMA roll-out, with London expanded to the right. Pilot and pilot control are the areas used in Dearden et al. (2009). Pre 04 indicates 1999 or 2000 (early) roll-out; 2004 indicates national roll-out. Our main analysis compares the red (pilot + matched) and blue (pilot control + matched) areas.

To guard against differential trends across areas in the absence of treatment, we implement a matching procedure to restrict the sample to 50 LAs with similar predicted probabilities of early EMA adoption (see Section III B below). We use nearest-neighbour matching to identify a sample of early and national roll-out areas with similar observable characteristics.[34] Through this matching process, we identified 16 LAs from each of the early roll-out and national roll-out areas, which we use in addition to the 18 handpicked areas from Dearden et al. (2009).

III B Estimation sample

Figure 1 shows the nine dark red pilot areas from Dearden et al. (2009) as well as the 16 lighter red areas that received the EMA before 2004 selected in our matching process. It also shows the dark blue pilot control areas and the lighter blue areas from our matching process. The green and yellow areas are left out from our main analysis; these were typically either rural national roll-out

[34]The matching was done using a probit model to calculate a propensity score for receiving the EMA prior to 2004. See Appendix B for more details.

areas (green) or urban early-treated areas (yellow) for which there was no close match.

Table 1: Sample descriptives (1987/88 and 1988/89 birth cohorts)

	Pilot + Matched		Whole of England			
	(1)	(2)	(3)	(4)	(5)	(6)
	FSM		FSM		All	
Roll-out area	Early	National	Early	National	Early	National
A. Student characteristics by age 16						
% White	75.76	80.33	60.11	79.27	74.33	87.50
% 5 A*-C at GCSE	25.32	24.16	29.99	26.02	48.36	55.79
% 5 A*-G at GCSE	74.23	74.81	77.27	75.94	87.60	91.13
% 5 A*-G but not 5 A*-C	48.79	50.55	47.12	49.85	39.18	35.31
% Special Educational Needs	30.27	30.48	29.87	32.19	18.83	15.87
% Deprived neighbourhood	54.24	44.28	66.02	33.00	38.56	10.85
% Crime by age 16	4.89	4.98	4.19	4.22	2.40	1.62
B. Student outcomes						
% Full-time Ed, age 16-17	55.89	55.65	60.89	57.87	68.62	72.53
% Part-time Ed, age 16-17	6.67	6.48	6.92	6.01	5.19	3.81
% Training, age 16-17	15.61	16.52	13.03	13.27	11.63	9.72
% NET, age 16-17	21.83	21.35	19.16	22.84	14.56	13.94
% Employed, age 17	10.41	10.59	8.30	12.34	12.69	16.05
% Crime age 16-18	7.15	7.60	6.38	6.67	3.94	2.98
% University attendance	17.86	17.02	25.02	18.19	35.35	37.73
Observations	30,004	25,004	80,450	73,427	352,461	753,443
Local Authorities	25	25	53	94	53	94

Notes: *Pilot + Matched* is the sample used in our main analysis. *Whole of England* is the whole country, but excluding 3 of the 150 LAs—see Appendix Section A for more details. We split each sample by timing of EMA receipt— *Early* for 1999 or 2000, and *National* for 2004. *FSM* = eligible for free school meals at age 15-16. The table shows pooled values for the 1987/88 and 1988/89 birth cohorts (that completed compulsory schooling in 2004 and 2005), the first two cohorts after the national roll-out of the EMA. Employed is based on earning above the lower earnings limit (£4,108 in 2004/05).

Table 1 confirms that early and national roll-out areas differed on a number of observable characteristics. Early roll-out areas were less white, both in the full sample and among those eligible for free school meals. Early roll-out areas had lower levels of prior attainment and higher rates of special educational needs in the full sample, but the reverse was true among those eligible for free school meals. Students in the early and national roll-out areas in the "pilot + matched" sample had similar demographic backgrounds and later-life outcomes, though somewhat fewer children lived in deprived neighbourhoods in the national roll-out areas.

III C Trends in outcomes

We assess the plausibility of the parallel trends assumption by examining differences in outcome trends between the national roll-out and always-treated groups. Following Kim and Lee (2019), we focus on *post*-treatment trends. Post-treatment trends will be parallel if (a) trends between the always-treated LAs and the national-roll-out LAs would have been the same in the absence of treatment, (b) treatment effects are constant across cohorts in the always-treated LAs, and (c) treatment effects are constant across cohorts in the national roll-out areas. (a) and (b) are closely related to the identification conditions; (c) is independently plausible if (b) holds (at least after a short implementation lag).[35]

We estimate event-study specifications of the form:

$$y_{ijc} = \alpha_j + \nu_c + \sum_{c \neq 2004}^{C} \beta_c \left(\mathbb{1}[cohort = c] \times ER_j \right) + \delta X_{ijc} + \epsilon_{ijc} \tag{2}$$

where α, ν and X are as in equation (1), $ER_j = 1$ in early roll-out areas, and $ER_j = 0$ if area j is a national roll-out area.[36] The coefficients $\hat{\beta}_c$ represent differences in trends between the early roll-out areas and the national roll-out areas. Figures 2 and 3 present event study estimates for 16 key outcomes.[37]

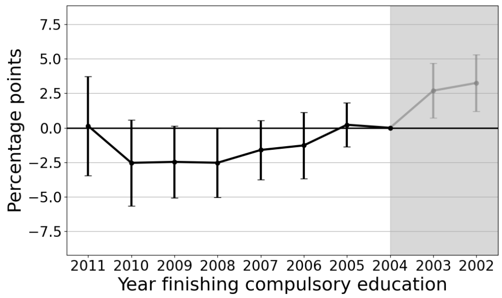
Without matching, post-trends in the early roll-out group—which includes a disproportionate number of urban LAs—often differ visibly from those in the national roll-out areas. One possible explanation is that other changes in the education system, such as sharp improvements in London school performance during the mid-2000s, affected urban LAs more than others. This reinforces the case for using matched LAs as our main analysis.

[35]Post-trends would also be parallel under the alternative assumption that treatment effects were constant within cohorts across areas, which corresponds to the identification assumption of Kim and Lee (2019). We cannot assess pre-treatment trends because administrative data is unavailable before the 2002 cohort. However, the value of these data would in any case be limited. Pre-trends would never be expected to be parallel in the "reverse difference-in-differences" framework of Kim and Lee (2019), and would only be expected to be parallel for two extra years in our framework even if we additionally assumed there were no implementation lags, as the EMA was rolled-out to most pre-2004 areas in 2000.

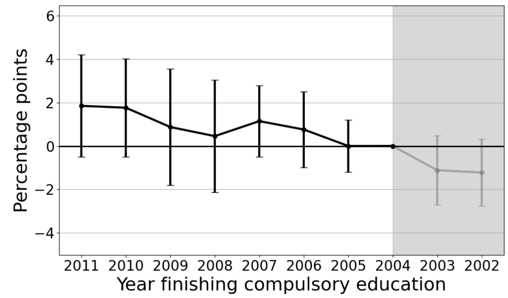
[36]We interact with ER_j instead of NR_j so that the deviations from the post-trends in the pre-period have the same sign as the estimated effects in our main regressions.

[37]See Online Appendix SA1-4 for equivalent figures by prior attainment.

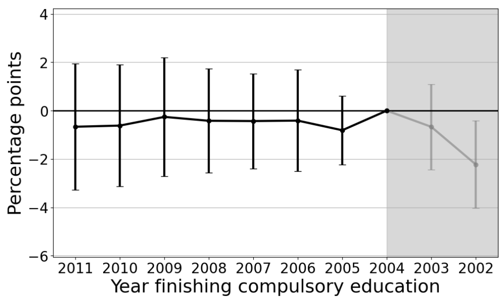
Figure 2: Area-by-cohort event studies



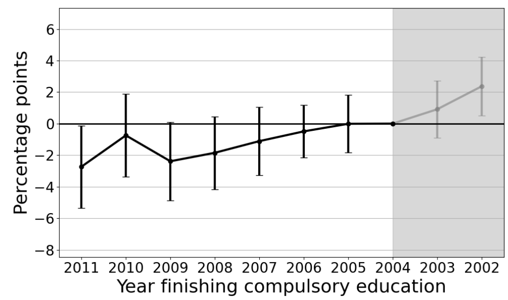
(a) Full-time education, age 16-17



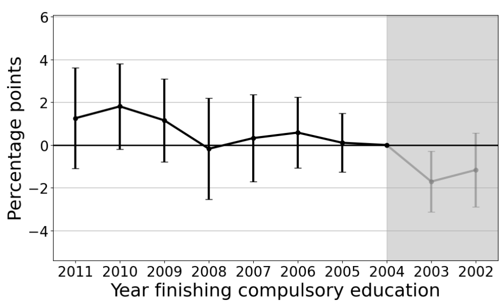
(b) Training, age 16-17



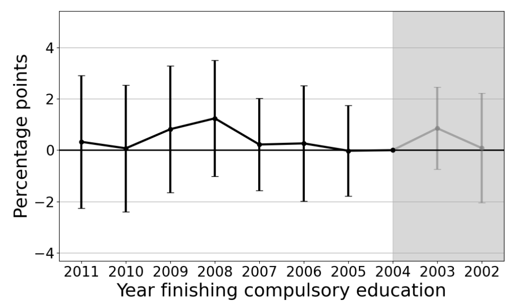
(c) Not in Education or Training, age 16-17



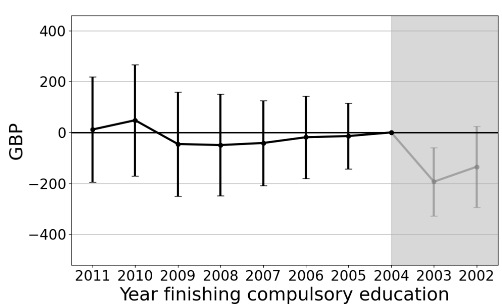
(d) Full-time education, age 17-18



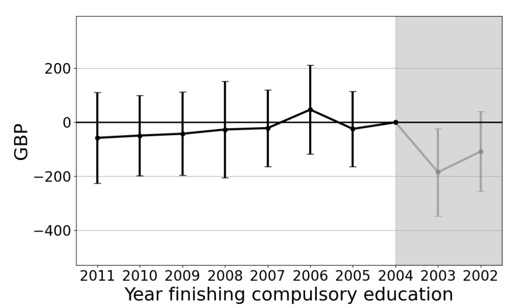
(e) Training, age 17-18



(f) Not in Education or Training, age 17-18



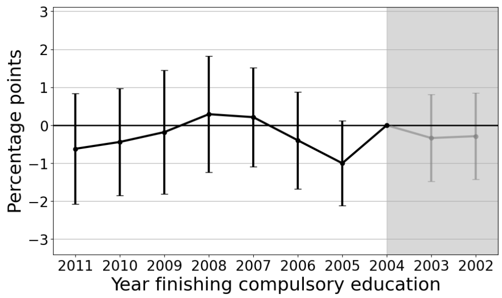
(g) Earnings, age 17



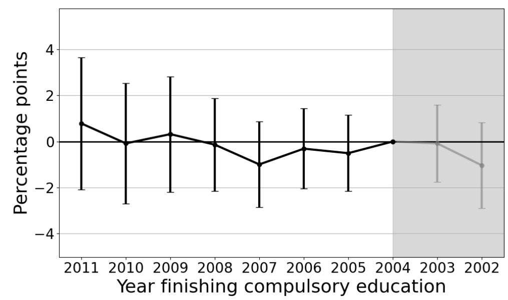
(h) Earnings if in full-time education, age 17

Notes: Event studies for key outcomes, with x-axis in reverse order. The unshaded area in each chart shows cohorts where we expect common trends (post-national roll-out). Shaded area covers cohorts which experience differences in EMA eligibility based on home LA. Error bars show 95% confidence intervals.

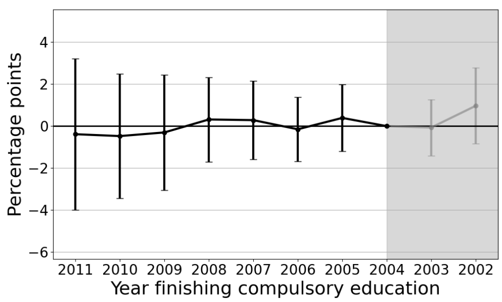
Figure 3: Area-by-cohort event studies (additional outcomes)



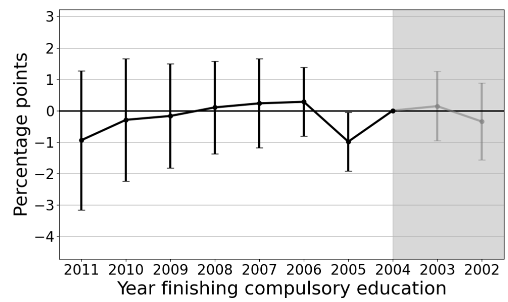
(a) Level 1 vocational qualification by age 26



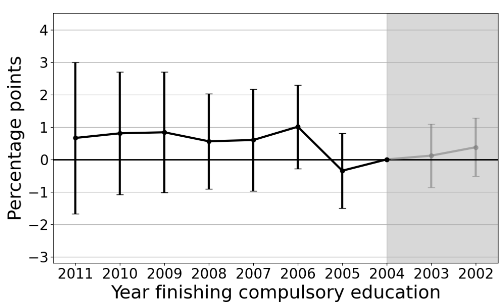
(b) Level 2 vocational qualification by age 26



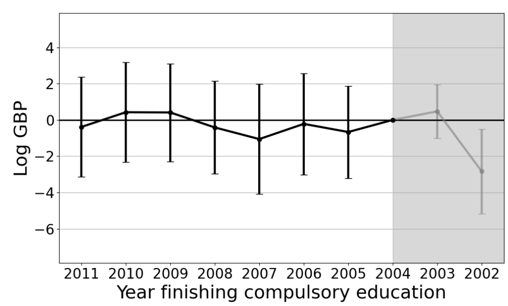
(c) Level 3 vocational qualification by age 26



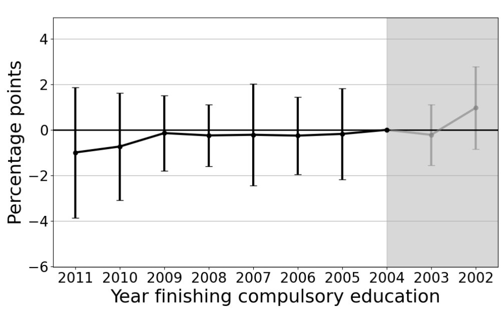
(d) Passed academic track



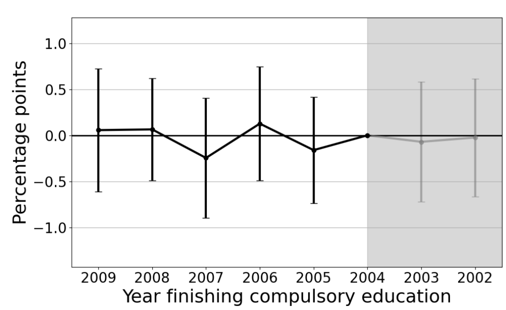
(e) University attendance



(f) Log earnings, age 20-25



(g) Benefit receipt, age 20-25



(h) % with conviction, age 16-17

Notes: See notes to Figure 2.

When using matched areas, post-roll-out trends are broadly parallel, suggesting that the identifying assumption is plausible in the matched sample. For effects on full-time education and training, we see some differences in post-period trends between early and national roll-out areas. If these differences reflect differences in the underlying trend in the areas of interest, it would suggest our estimates of the effect of the EMA on full-time education could be biased upwards.

Rather than testing for trend significance, which has recently been criticised (Roth, 2022), we instead check whether our estimates of the effects on full-time education and training are robust to using a synthetic difference-in-difference approach (Arkhangelsky et al., 2021). This approach reweights the control group to minimise differences in post-treatment trends between treated and control areas. Results can be found in Table 4. Estimated effect sizes using this approach are very similar to those for the standard difference-in-differences, suggesting the results are not driven by a failure of the common trends assumption.

IV Results

IV A Economic activity of 16-18-year-olds

This section investigates the impact of the EMA on activities in the first and second academic years after compulsory schooling (Year 12 at age 16-17 and Year 13 at age 17-18). We assess the effect on full-time and part-time education participation, training, and the residual category (Not in education or training, or NET). We also divide NET into NEET (not in education, employment or training) and employment only, although we can only approximate this split due to the misalignment of the academic year and the tax year.[38] Finally, we investigate the impact of the EMA on the labour supply of 17-year-olds, based on earnings in the tax year that overlaps Year 12 and Year 13. Throughout, we show results for students eligible for free school meals, overall and split into prior attainment groups as defined in Section I A. [39]

Table 2 presents estimates of the impact of the EMA on education participation and related activities at age 16-17 (Year 12). We estimate that education participation rose by just under 3 percentage points as a result of the policy, on a baseline of 51% (row *a*). This estimate is slightly less than half the estimated participation effect in Dearden et al. (2009); see Section IV A 1 for a discussion of these differences. The effects are slightly larger in the low and medium prior attainment groups than for the high prior attainment group; they are much larger relative to the baseline participation level for these groups. Most of the increase in participation is driven by individuals moving into further education colleges rather than schools, especially among those with low and medium prior attainment (rows *b* and *c*). This was likely because schools had stricter

[38]For example, we define NEET at age 16-17 (Year 12) based on activity in Year 12 and earnings in the tax year that runs from April of Year 11 and April of Year 12. Individuals that are not in education or training and earn less than the LEL (£4,108 in the 2004/05 tax year; see Section II) are classified as NEET. Individuals that are not in education or training and earn more than the LEL are classified as in employment only. Because not all individuals can be matched to HMRC data and no HMRC data is available for the 2002/03 tax year, the sample for estimating the effect of the EMA on NEET and Employment is smaller and the effect sizes do not sum to the NET effects.

[39]We find no evidence that the EMA impacted the number of people in each prior attainment group (see Section V A).

entry requirements (commonly requiring that students have at least 5 A*-C grades at GCSE, which is also the level we use to define the high prior attainment group) and typically offered a narrower range of qualifications (primarily A-Levels).

Rows *d* to *f* show the activities these additional full-time students are drawn from. Although the estimates are not statistically significant, they suggest around 40% drawn from training, 20% from part-time education and 40% from NET. This broadly holds across the different prior attainment groups, although there is a greater proportion drawn from training among those with high prior attainment and a greater proportion drawn from NET among those with low prior attainment. The reduction in NET appears to come primarily from people who would otherwise have been in employment rather than NEET (rows *g* and *h*), although our measures of employment and NEET are imperfect.[40]

Table 3 presents equivalent estimates for age 17-18 (Year 13). The overall impact on participation is smaller at 1.6 percentage points (on a baseline of 38%). Unlike at age 16-17, this effect is entirely driven by those with high and medium prior attainment. Around half of these individuals are drawn from training and the rest from employment and part-time education.

There was no impact on full-time education at age 17-18 for those with low prior attainment. In fact, these individuals were significantly *less* likely to be involved in any form of education or training by age 17-18 and more likely to be NEET (by 3.4 percentage points, on a baseline of 44%). The EMA thus appears to have been successful at keeping people with very low GCSE attainment in formal education at age 16-17 but at the cost of increasing their inactivity rates at age 17-18, perhaps because many dropped out of education at a non-standard point in the year and then found it harder to find work or training.[41]

[40]The result of no overall reduction in the NEET rate is consistent with a high and stagnant national NEET rate among 16-18 year olds throughout the 2000s according to official statistics (Public Health England, 2014).

[41]We are unable to test this directly with the data we have, which does not enable us to observe precise timing of dropout.

Table 2: Impact of the EMA on the main economic activities of FSM-eligible 16-17 year olds

		Prior attainment		
	(1)	(2)	(3)	(4)
	All FSM	Low	Medium	High
a. Full-time education	2.86***	3.03**	2.98**	2.26**
	(0.92)	(1.48)	(1.29)	(1.10)
	50.97	*26.18*	*50.40*	*82.69*
Of which:				
b. School	0.41	-0.25	0.40	0.99
	(0.53)	(0.54)	(0.57)	(1.34)
	13.58	*4.23*	*9.69*	*34.05*
c. Further Education	2.44***	3.29**	2.57**	1.27
	(0.82)	(1.52)	(1.14)	(1.27)
	37.40	*21.95*	*40.71*	*48.64*
d. Part-time education	-0.62	-0.76	-0.44	-0.89
	(0.56)	(1.27)	(0.62)	(0.64)
	6.49	*8.25*	*6.59*	*3.72*
e. Training	-1.22	-0.59	-1.40	-1.34*
	(0.74)	(1.50)	(0.85)	(0.67)
	18.29	*24.76*	*19.98*	*7.06*
f. Not in ed. or training	-1.01	-1.68	-1.14	-0.04
	(0.68)	(1.22)	(0.92)	(0.63)
	24.25	*40.80*	*23.04*	*6.54*
Of which:				
g. Employment	-0.23	-0.08	-0.32	-0.13
	(0.18)	(0.36)	(0.26)	(0.22)
	1.36	*1.31*	*1.70*	*0.62*
h. NEET	0.05	0.01	0.01	-0.19
	(0.78)	(1.55)	(1.01)	(0.69)
	20.12	*35.13*	*18.84*	*5.30*
Observations *(a.-f.)*	108,660	26,200	55,485	25,065
Observations *(g.-h.)*	76,255	19,415	38,275	18,445
Number of clusters	50	50	50	50

Notes: Sample consists of FSM-eligible young people who were resident in *Pilot+Matched* LAs and finished compulsory schooling between 2002 and 2005. All means and coefficients are multiplied by 100 so that they can be interpreted as percentages and percentage point changes. Employment (earning above the LEL and not being in education or training) and NEET (earning below the LEL and not being in education or training) are based on earnings in the tax year that runs from April of Year 11 through to April of Year 12. Hence *g* and *h* do not sum perfectly to *f*, and the sample sizes are different. Low attainment means fewer than five GCSEs at grade G or above, medium attainment means five GCSEs at grade G or above but fewer than five at grade C or above, and high attainment means at least five GCSEs at grade C or above. Standard errors clustered at the LA level are shown in parenthesis. The mean of the dependent variable across individuals in the two pre-roll-out cohorts in the national roll-out LAs is given in italics. *,** and *** indicate significance at the 90%, 95%, and 99% confidence level.

Table 3: Impact of the EMA on the main economic activities of FSM-eligible 17-18 year olds

		Prior attainment		
	(1)	(2)	(3)	(4)
	All FSM	Low	Medium	High
a. Full-time education	1.61**	-0.21	1.99*	2.85**
	(0.70)	(1.03)	(1.07)	(1.21)
	38.01	*18.70*	*34.70*	*69.76*
Of which:				
b. School	0.21	0.29	-0.21	0.99
	(0.40)	(0.45)	(0.46)	(1.15)
	8.30	*2.10*	*3.91*	*26.25*
c. Further Education	1.40*	-0.50	2.19**	1.86
	(0.72)	(0.85)	(1.09)	(1.30)
	29.71	*16.60*	*30.79*	*43.52*
d. Part-time education	-0.57*	-0.87	-0.63	-0.38
	(0.33)	(0.78)	(0.47)	(0.70)
	7.93	*9.96*	*7.64*	*6.07*
e. Training	-1.54**	-1.68	-1.53*	-1.43
	(0.64)	(1.20)	(0.87)	(0.86)
	18.67	*19.60*	*21.77*	*10.08*
f. Not in ed. or training	0.49	2.76**	0.17	-1.04
	(0.63)	(1.24)	(0.83)	(0.68)
	35.38	*51.75*	*35.88*	*14.09*
Of which:				
g. Employment	-0.64*	-0.38	-0.47	-1.35***
	(0.33)	(0.66)	(0.38)	(0.47)
	5.93	*5.52*	*7.00*	*4.02*
h. NEET	1.15*	3.44**	0.59	0.26
	(0.63)	(1.29)	(0.85)	(0.66)
	27.07	*43.61*	*26.76*	*8.30*
Observations *(a.-f.)*	108,660	26,200	55,485	25,065
Observations *(g.-h.)*	100,105	23,875	51,155	23,435
Number of clusters	50	50	50	50

Notes: See notes to Table 2.

As noted in Section III C, event studies suggest potentially different underlying trends in the education and training outcomes between the treatment and matched control areas. In Table 4 we show that the results for these outcomes are robust to using a synthetic difference-in-differences estimator (Arkhangelsky et al., 2021). This approach chooses a weighted group from the full set of potential control LAs in order to match post-treatment trends. The re-weighted sample shows no substantial difference in post-trends for education and training outcomes. The headline results for education participation and training are very similar as in our main specification, which reassures us that the estimated effects are not driven by a failure of the common trends assumption.

Table 4: Robustness of education and training effects

	Age 16-17		Age 17-18	
	(1) FT Education	(2) Training	(3) FT Education	(4) Training
Main Specification				
Impact of the EMA (β)	2.86***	-1.22	1.61**	-1.54**
	(0.92)	(0.74)	(0.70)	(0.64)
Observations	108,660	108,660	108,660	108,660
Synthetic Difference-in-differences				
Impact of the EMA (β)	3.35***	-1.25*	1.99**	-0.97*
	(1.11)	(0.66)	(0.88)	(0.53)
Observations	265,150	265,150	265,150	265,150

Notes: The main specification is as in Tables 2 and 3, which uses the matched LAs as controls. The synthetic difference-in-differences approach uses a weighted average of *all* control LAs (hence the larger sample sizes).

Table 5 shows the impact of the EMA on the earnings of 17-year-olds (that is, earnings in the tax year that runs from April of Year 12 to April of Year 13). We find a significant overall drop in earnings of £153 per year (in 2023/24 prices), on a baseline of £2200 (row *a*). The effect is similar for the low prior attainment group (but against a lower baseline), smaller for the medium prior attainment group, and larger for the high prior attainment group (around £400 on a baseline of £2500). These differences by prior attainment are likely partly explained by differences in full-time education participation and therefore in EMA receipt across groups. Per full-time student in Year 12, earnings declined by around £300 overall, £600 per low-attaining full-time student, £100 per medium-attaining full-time student, and £500 per high-attaining full-time student. For someone earning £4.81 per hour (the 2005 minimum wage for 17-year-olds in 2023/24 prices), the average decline per full-time student would imply around 62 fewer hours worked per year (or one fewer hour per week). These estimates capture effects at the intensive and extensive margins, as the sample includes individuals with zero earnings.[42] Row *b* shows that there is a similar pattern of effects for the share of young people earning more than the LEL, providing reassurance that these effects are not driven by differential changes in employers' voluntary reporting of 17-year-olds' earnings.

[42]This is important given that only around one in three individuals in our sample have earnings above the LEL.

Table 5: Effect of the EMA on the earnings of FSM-eligible 17-year-olds

		Prior Attainment		
	(1)	(2)	(3)	(4)
	All FSM	Low	Medium	High
a. Earnings	-153***	-171*	-56	-395***
	(55)	(86)	(75)	(108)
	2,168	*1,489*	*2,373*	*2,522*
b. Employment	-1.05**	-0.97	-0.61	-2.29**
	(0.43)	(0.70)	(0.52)	(0.95)
	12.25	*8.58*	*13.84*	*12.85*
For those in full-time education:				
c. Earnings	-130**	0	-29	-287***
	(48)	(125)	(63)	(86)
	1,322	*759*	*1,176*	*1,683*
Observations *(a.-b.)*	100,105	23,875	51,155	23,435
Observations *(c.)*	42,685	4,735	20,250	17,390
No. of clusters	50	50	50	50

Notes: Sample consists of FSM-eligible young people who were resident in *Pilot+Matched* LAs and finished compulsory schooling between 2002 and 2005. Rows *a* and *c* show estimates in levels, with monetary values in 2023/24 prices. Row *b* is the percentage point change in the probability of earnings above the lower earnings limit (LEL), which was £4,108 in the 2004/05 tax year, which we treat as a measure of employment. Earnings are Winsorized at the 99th percentile. Low attainment means fewer than five GCSEs at grade G or above, medium attainment means five GCSEs at grade G or above but fewer than five at grade C or above, and high attainment means at least five GCSEs at grade C or above. Standard errors clustered at the LA level are shown in parenthesis. The mean of the dependent variable across individuals in the two pre-roll-out cohorts in the national roll-out LAs is given in italics. *,** and *** indicate significance at the 90%, 95%, and 99% confidence level.

The effects on earnings could be driven by people switching from paid work or training to (unpaid) education, or by people working less part-time who would have been in education anyway. We investigate this by restricting the sample to individuals in full-time education in Year 13; row *c* shows an average drop in earnings conditional on full-time education participation of £130 per year. This estimate could be biased by composition effects (driven by individuals switching into education having different part-time earnings from those who would have been in education in the absence of the EMA). However, we see that the overall average drop in earnings of £130 is driven by a larger drop of around £300 for those with high prior attainment, even though our estimates suggest that relatively few of those students were induced to stay in education by the EMA. This supports the conclusion that the EMA substantially reduced part-time work alongside study among those with high prior attainment.

IV A 1 Comparison with previous estimates

We estimate that full-time education participation at age 16-17 increased as a result of the EMA by 2.9 percentage points for FSM-eligible students (SE: 0.9). This effect is considerably smaller than estimates from the evaluations of the pilot phase of the EMA (Dearden et al., 2009; Middleton et al., 2005). Dearden et al. (2009) estimate a 6.7 percentage point increase (SE: 1.7) in full-time participation at age 16-17 among those eligible for the full EMA, statistically significantly larger at the 95% confidence level.[43]

There are several potential explanations for this disparity. First, our analysis on the full population provides tentative evidence that the effect on participation at age 16-17 was slightly larger for students who were eligible for the full EMA award but not free school meals (around 3.5 percentage points; see Section V A). Second, we employ a difference-in-differences approach, which is likely to better capture differences between areas than the cross-sectional approaches used in the pilot evaluations. Third, the pilot evaluations use data from the cohort that finished compulsory schooling in 1999, five years before the national roll-out of the policy in 2004, which we are evaluating. During that period, the amount of cash support available was not increased, meaning it declined in real value.

Fourth, the survey data used in pilot evaluations may capture participation differently from our administrative data. Part of this could be error in survey responses; in particular, responses may have been influenced by the existence of the EMA itself, which may have biased estimates in Dearden et al. (2009) upward.[44] However, surveys might also better capture actual engagement in education rather than just enrolment. For example, a student who enrolled on an education course but stopped attending midway through may appear as in education in the administrative data but as NEET in the survey data if the survey was conducted after they had stopped attending. If the EMA reduced instances of enrolling and then dropping out, it would thus be detectable in the survey data but not the administrative data. This would be consistent with Dearden et al.'s (2009) finding that around three-quarters of the increase in participation came from the NEET population, while our estimates suggest that there was no reduction in NEET shares.

IV B Educational attainment

Since the aim of the EMA was not only to extend time in education but also to improve students' qualifications and life chances, a key question is whether it succeeded in raising educational attainment among eligible students. Table 6 presents the impact of the EMA on vocational and academic qualifications, including progression to and graduation from university. For vocational qualifications, we examine whether individuals passed Level 1 (an entry-level qualification), Level 2 (a mid-level qualification broadly equivalent to achieving a grade C or above at GCSE), or Level 3 (a higher-level technical qualification, roughly equivalent to A-Levels in the UK or high school in the United States) by age 23. On the academic side, we consider whether individuals passed A-Levels (the main qualification enabling university entry) by age 23,

[43]Given independent samples, the standard error for the difference between the two estimates is $\sqrt{0.9^2 + 1.7^2} \approx 1.9$. The *p-value* for the null that the two estimates are the same is therefore 0.047.

[44]In practice, many "full-time" courses for 16-18 year-olds in this period were less intensive than compulsory schooling and included several free periods per week. This may have led some students in full-time education to mistakenly report that they were in part-time education. Since the EMA was only paid to individuals in full-time education, this misreporting was plausibly less likely among EMA recipients.

progressed to university by age 23, and graduated from university by age 26. Among those who pursued A-Levels or vocational equivalents, we also assess whether the EMA affected standardised A-Level points scores.

We find no meaningful overall effect of the EMA on any of the attainment measures we study. However, these overall null effects mask meaningful impacts on students with high prior attainment. These students were nearly 3 percentage points more likely to pass their A-Levels and achieved A-Level scores around 7% of a standard deviation higher, consistent with the reduction in working while studying shown in Table 5.[45] They were 3.6 percentage points more likely to enter university but not significantly more likely to graduate. High-attaining students were also less likely to obtain lower-level vocational qualifications, likely reflecting substitution away from work-based training.

The confidence interval for the overall effect on university graduation rates is $[-1.0, 0.6]$ percentage points, allowing us to rule out increases larger than 0.6 percentage points (5% of the baseline rate of 11.6%). Among students with high prior attainment, the picture is different: the EMA increased university enrolment by 3.6 percentage points but the effect on *graduation* (0.9 percentage points, with a confidence interval of $[-1.4, 3.2]$) is too imprecisely estimated to distinguish between no effect and a meaningful increase of up to 8% of the baseline graduation rate.

[45] Compositional effects are unlikely to explain this result, as we estimate that the EMA had no effect on the share of students taking A-Levels (the coefficient estimate is 0.38 percentage points, with a standard error of 0.44). If anything, the estimated impact on A-Level scores might be attenuated by up to a quarter due to standardising of exam results within cohorts.

Table 6: Effect of the EMA on qualifications of FSM-eligible students

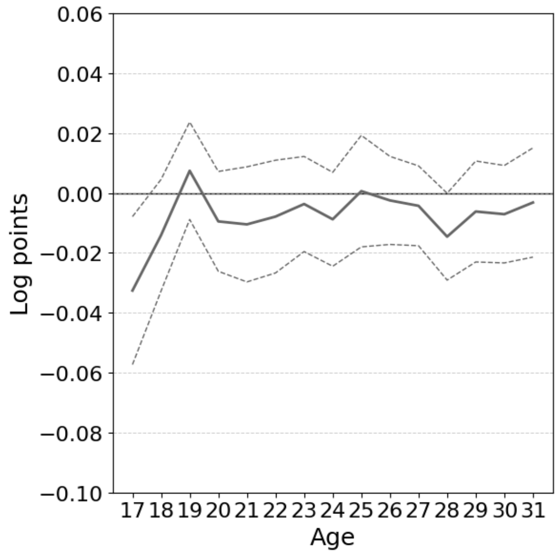
		Prior attainment		
	(1)	(2)	(3)	(4)
	All FSM	Low	Medium	High
A. Vocational qualifications				
a. Level 1 (basic)	0.19	0.44	0.67	-1.30*
	(0.44)	(0.98)	(0.56)	(0.72)
	11.45	*17.76*	*10.86*	*5.55*
b. Level 2 (intermediate)	-0.32	-0.49	0.42	-2.45*
	(0.70)	(0.97)	(1.21)	(1.30)
	29.41	*24.22*	*35.28*	*21.56*
c. Level 3 (high school equiv.)	0.20	-0.51	-0.18	1.65
	(0.65)	(0.64)	(1.02)	(1.57)
	17.28	*4.23*	*18.35*	*30.55*
B. Academic qualifications				
d. Passed academic track	0.45	0.11	0.12	2.91**
	(0.54)	(0.08)	(0.66)	(1.43)
	11.87	*0.15*	*4.01*	*46.01*
e. University enrolment	0.45	-0.08	-0.27	3.62***
	(0.35)	(0.30)	(0.46)	(1.19)
	14.99	*1.53*	*8.50*	*47.63*
f. University degree	-0.21	-0.28	-0.29	0.92
	(0.41)	(0.23)	(0.44)	(1.16)
	11.61	*1.03*	*5.71*	*39.41*
g. Standardised score,	0.05*	-	0.02	0.07**
academic track	(0.03)		(0.05)	(0.03)
	-0.55		*-1.15*	*-0.32*
Observations *(a.-f.)*	108,660	26,200	55,485	25,065
Observations *(g.)*	19,985		5,415	14,385
Number of clusters	50	50	50	50

Notes: Sample consists of FSM-eligible young people who were resident in *Pilot+Matched* LAs and finished compulsory schooling between 2002 and 2005. Means and coefficients in rows *a* to *f* are multiplied by 100 so that they can be interpreted as percentages and percentage point changes. Scores in row *g* are standardised so that results for all students in each cohort have a mean of zero and a standard deviation of one. Low attainment means fewer than five GCSEs at grade G or above, medium attainment means five GCSEs at grade G or above but fewer than five at grade C or above, and high attainment means at least five GCSEs at grade C or above. Standard errors clustered at the LA level are shown in parenthesis. The mean of the dependent variable across individuals in the two pre-roll-out cohorts in the national roll-out LAs is given in italics. *,** and *** indicate significance at the 90%, 95%, and 99% confidence level. A dash (-) indicates suppression for statistical disclosure reasons.

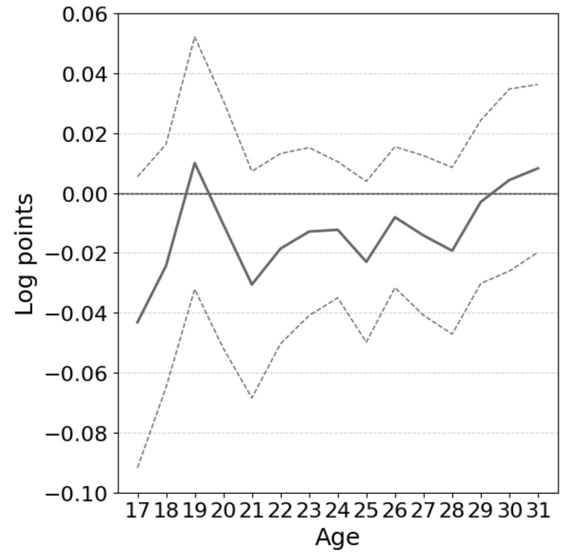
IV C Longer-term labour market outcomes

We next examine whether the EMA had any effects on students' economic outcomes later in life. We observe earnings and welfare benefit receipt up to age 31. Table 7 reports estimates of the programme's impact on earnings, employment and welfare benefit receipt averaged over three age ranges (20–25 and 26–31, and 20-31), overall and split by prior attainment. Then Figure 4 shows impacts on earnings and employment each age from 17 through to 31 (overall and for those with high prior attainment only).

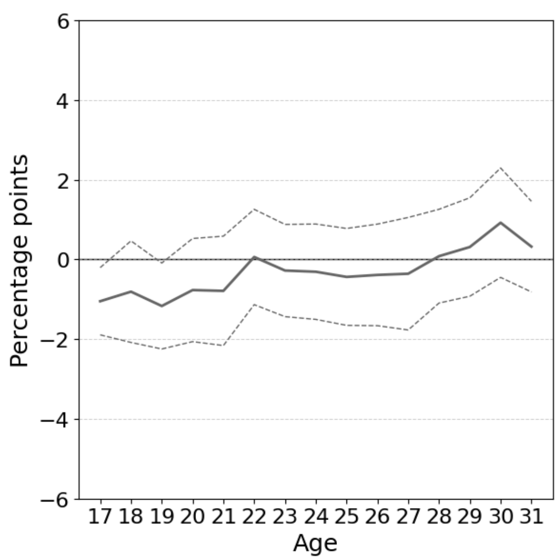
Figure 4: Impact on earnings and employment of FSM-eligible students by age



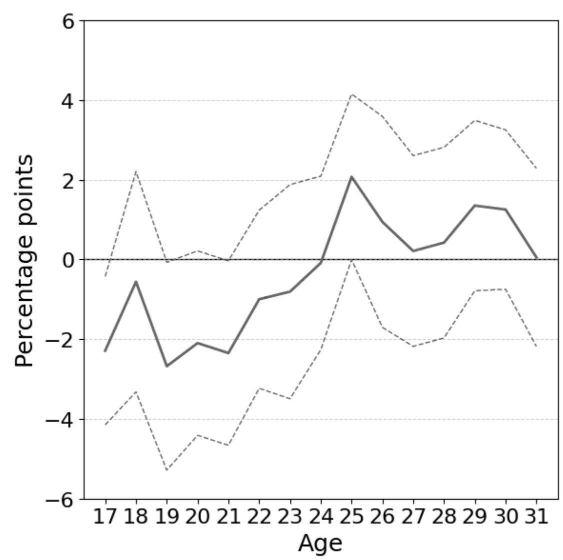
(a) All FSM-eligible students: log earnings



(b) FSM with high prior att.: log earnings



(c) All FSM-eligible students: employment



(d) FSM with high prior att.: employment

Notes: Sample consists of FSM-eligible young people who were resident in *Pilot+Matched* LAs and finished compulsory schooling between 2002 and 2005. Panels (a) and (c) show results for all individuals. Panels (b) and (d) show results for the high attainment group (those who achieved at least 5 A*-C in their GCSE exams). Gray dashed lines indicate 95% confidence intervals based on standard errors clustered at the LA level. Employment is measured as the proportion of students earning above the lower earnings limit (LEL), which was £4,108 in the 2004/05 tax year

Table 7: Effect of the EMA on labour market outcomes of FSM-eligible students

		Prior Attainment		
	(1)	(2)	(3)	(4)
	All FSM	Low	Medium	High
a. Log earnings,	-0.59	-2.17	0.49	-1.73*
age 20-25	(0.61)	(1.55)	(0.77)	(1.01)
	17,178	*14,087*	*17,192*	*19,324*
b. Log earnings,	-0.70	-0.39	-0.69	-0.68
age 26-31	(0.58)	(1.80)	(0.80)	(1.07)
	21,017	*17,513*	*20,092*	*25,208*
c. Log earnings,	-0.69	-1.30	-0.22	-1.15
age 20-31	(0.53)	(1.39)	(0.70)	(0.89)
	19,187	*15,842*	*18,734*	*22,375*
d. Employment,	-0.42	-0.42	-0.31	-0.71
age 20-25	(0.46)	(0.99)	(0.60)	(0.80)
	43.50	*27.37*	*44.44*	*59.64*
e. Employment,	0.15	0.55	-0.31	0.70
age 26-31	(0.54)	(0.97)	(0.75)	(0.97)
	52.83	*32.04*	*54.55*	*73.41*
f. Employment,	-0.14	0.07	-0.31	-0.01
age 20-31	(0.45)	(0.90)	(0.59)	(0.77)
	48.16	*29.70*	*49.50*	*66.52*
g. Out of work benefit,	0.72	2.11**	0.39	-0.27
age 20-25	(0.52)	(1.00)	(0.66)	(0.57)
	28.10	*48.78*	*25.42*	*9.60*
h. Out of work benefit,	0.75*	1.49	0.68	-0.03
age 26-31	(0.43)	(0.95)	(0.59)	(0.53)
	22.62	*40.12*	*19.93*	*7.63*
i. Out of work benefit,	0.74	1.80*	0.53	-0.15
age 20-31	(0.45)	(0.90)	(0.59)	(0.49)
	25.36	*44.45*	*22.68*	*8.61*
Observations *(a.)*	253,560	36,460	132,600	81,745
Observations *(b.)*	316,060	45,235	166,145	101,870
Observations *(c.)*	569,620	81,695	298,750	183,615
Observations *(d.-e.)*	600,620	143,245	306,930	140,600
Observations *(f.)*	1,201,235	286,490	613,860	281,195
Observations *(g.-h.)*	651,955	157,190	332,915	150,400
Observations *(i.)*	1,303,910	314,375	665,830	300,805
No. of clusters	50	50	50	50

Notes: Sample consists of FSM-eligible young people who were resident in *Pilot+Matched* LAs, finished compulsory schooling between 2002 and 2005. Estimates in rows *a* to *c* are log point differences in annual earnings over the indicated age range, in years in which individuals earned over the Lower Earnings Limit. Estimates in rows *d* to *i* can be interpreted as percentage point changes in the probability of earning over the Lower Earnings Limit - used as a measure of employment - or having least one spell on out-of-work welfare benefits lasting at least six months in any given fiscal year. Low attainment means fewer than five GCSEs at grade G or above, medium attainment means five GCSEs at grade G or above but fewer than five at grade C or above, and high attainment means at least five GCSEs at grade C or above. Standard errors clustered at the LA level are shown in parenthesis. The mean of the dependent variable across individuals in the two pre-roll-out cohorts in the national roll-out LAs is given in italics (monetary values are in 2023/24 prices). For log earnings, the exponential of the mean is given instead, and so should be interpreted in GBP. *,** and *** indicate significance at the 90%, 95%, and 99% confidence level.

Rows *a* to *c* of Table 7 show no evidence that the EMA improved the earnings outcomes of eligible individuals through their twenties. Panel (a) of Figure 4 shows that this is overall impact is very stable (at around -0.7%, but statistically insignificantly different to zero) through people's twenties and up to age 31. We can reject average annual earning gains larger than 0.3% between ages 20 and 31 with 95% confidence. Rows *d* - *f* show no overall increase in employment (Figure 4 shows this is stable by age), while rows *g* - *i* show no overall reduction in the share of individuals claiming out-of-work-related welfare payments. The confidence interval for the employment effect over the same period rules out employment gains above 0.7% (1.5% of the baseline employment rate).

Again, there are important differences by prior attainment. Among students with high prior attainment, there is no overall impact on employment or welfare receipt, but there is a statistically significant reduction in earnings of around 1.7% per year between ages 20 and 25. This is probably explained by this groups' increased university participation; we also see evidence in Panel (d) of Figure 4 that employment rates were lower during individuals' early twenties. We do not see strong evidence of a delayed improvement in labour market outcomes: row (c) of column (4) suggests the overall impact of the policy on earnings remains slightly (though insignificantly) negative on average between ages 26 and 31 for this group. Panel (b) of Figure 4 provides some tentative evidence of earnings catching up by individuals' early thirties; the point estimate is marginally positive at ages 30 and 31, but small and insignificantly different to zero.

For those with low prior attainment, we also observe negative (but statistically insignificant) effects on earnings between ages 20 and 25, but impacts that are close to zero between 26 and 31. Although impacts on working are small, we find evidence of small increases in the share of individuals claiming out-of-work welfare of around 2 percentage points per year on average. Given this does not correspond to lower employment rates, a possible explanation is that the EMA increased knowledge of or willingness to engage with available government support.

IV D Crime

There are several channels through which the programme might have influenced criminal behaviour. By increasing time spent in education, the EMA may have reduced idle time during the school day and limited opportunities to engage in criminal activity. It may also have lowered the incentive to commit financially motivated crimes through wealth effects. On the other hand, providing teenagers with cash could have increased spending on items associated with risky behaviours, such as drugs, which may have increased criminal convictions, including among non-recipients.

Rows *a* to *c* of Table 8 report the impact of the EMA on criminal convictions during the ages of EMA eligibility (ages 16–18), while rows *d* to *f* show impacts during participants' twenties (ages 19–29). We present estimates across all crimes and for two major offence categories: drug offences and theft. We find no overall impact of the EMA on criminal convictions, although the confidence interval at ages 19–29 is $[-0.47, 0.03]$ percentage points, meaning we can rule out increases in crime but not modest reductions of up to 13% of the baseline rate. This masks substantial heterogeneity by prior attainment. For those with low prior attainment, we observe some reductions in criminal activity. During the eligibility period (ages 16–18), convictions fell by 0.8 percentage points, though this estimate is imprecise and not statistically significant. Between ages 20 and 29, we estimate a statistically significant drop in convictions of 0.9 percentage points, of which around one-third is attributable to theft. These represent a fall of around 10% in conviction rates, equivalent to about 200 fewer convictions per cohort annually.[46]

Table 8: Impact of the EMA on criminal convictions of FSM-eligible students

		Prior Attainment		
	(1)	(2)	(3)	(4)
	All FSM	Low	Medium	High
a. Conviction,	-0.07	-0.80	0.38	-0.30*
age 16-18	(0.26)	(0.71)	(0.24)	(0.17)
	4.47	*11.38*	*3.04*	*0.83*
b. Drug Conviction,	-0.03	-0.11	-0.02	-
age 16-18	(0.06)	(0.20)	(0.06)	-
	0.42	*1.20*	*0.25*	-
c. Theft Conviction,	-0.09	-0.39	0.03	-0.06
age 16-18	(0.11)	(0.36)	(0.11)	(0.09)
	1.21	*3.55*	*0.67*	*0.13*
d. Conviction,	-0.22	-0.90**	0.03	-0.14
age 19-29	(0.13)	(0.35)	(0.14)	(0.09)
	3.74	*8.16*	*3.03*	*0.90*
e. Drug Conviction,	-0.02	-0.09	-0.01	0.00
age 19-29	(0.03)	(0.08)	(0.03)	(0.03)
	0.48	*1.03*	*0.41*	*0.09*
f. Theft Conviction,	-0.04	-0.33*	0.09	-0.06*
age 19-29	(0.06)	(0.19)	(0.06)	(0.03)
	0.73	*1.99*	*0.44*	*0.12*
Observations *(a.-c.)*	213,510	52,394	110,980	50,136
Observations *(d.-f.)*	1,067,550	261,970	554,900	250,680
No. of clusters	50	50	50	50

Notes: Sample consists of FSM-eligible young people who were resident in *Pilot+Matched* LAs and finished compulsory schooling between 2002 and 2005. Each individual is in the sample once per relevant school year (so twice for the 16-18 estimates and ten times for the 19-29 estimates). Estimates can be interpreted as percentage point changes in the probability of being convicted of a crime per year during the indicated range. Low attainment means fewer than five GCSEs at grade G or above, medium attainment means five GCSEs at grade G or above but fewer than five at grade C or above, and high attainment means at least five GCSEs at grade C or above. Standard errors clustered at the LA level are shown in parenthesis. The mean of the dependent variable across individuals and ages in the two pre-roll-out cohorts in the national roll-out LAs (which can be interpreted as the rate of having any conviction in a year) is given in italics. *,** and *** indicate significance at the 90%, 95%, and 99% confidence level. A dash (-) indicates suppression for statistical disclosure reasons.

We observe no changes in conviction rates for students with medium and high prior

[46]There are around 20,000 FSM-eligible students with low prior attainment per cohort.

attainment that are significant at the 95% confidence level. For the high-attainment group, a fall in overall convictions at age 16-18 and a reduction in theft convictions at age 19-29 are significant at the 90% confidence level, but these results are not robust to alternative specifications (see Section V A and Table A5). In contrast, the results for students with low prior attainment are consistent across specifications.

The EMA's long-run impact on criminal convictions of those with low prior attainment could be due to path dependence in criminal trajectories (see, for example, Bell et al., 2018; Arora, 2023). Alternatively, time spent in education—regardless of formal qualifications—may have offered structure, adult supervision, or social engagement that altered life paths in other lasting ways. Receiving state support at a formative age may also have shifted perceptions of social norms and entitlements. This could, for example, have made individuals more likely to claim welfare benefits and less likely to engage in economically motivated crime. While these mechanisms are difficult to disentangle in our data, the patterns suggest that traditional measures of human capital accumulation may understate some of the social returns to programmes like the EMA.

V Robustness and additional analysis

V A Robustness checks

Our results are robust to a battery of checks. Since we have a large number of outcomes, with results split by prior attainment, we present our main robustness checks across Appendix tables A3, A4 and A5. First, we test the sensitivity of our estimates to excluding controls for GCSE attainment. These controls could, in principle, be endogenous if the EMA affected performance at age 16. However, dropping them has virtually no effect on our estimates.[47] Second, we re-estimate our models excluding the 12 local authorities among the 25 early roll-out areas in our main estimation sample that received a different variant of the EMA prior to the national roll-out. In some of these areas, bonuses were larger; in others, the award was paid to parents rather than directly to the child; in others, the maximum allowance was up to £40 per week instead of £30. Excluding areas with these variants does not substantially affect our results. Third, we exclude the cohort for which compulsory schooling ended in 2004—the first after the national roll-out—and instead include the 2006 cohort. If there had been a lag in the implementation of the EMA, dropping this cohort should have led to larger estimates. In fact, we find that our results are substantially unchanged.

Fourth, we re-estimate our models using all LAs, rather than restricting to the matched sample. As discussed earlier, we focus on the matched sample because the common trends assumption appears more plausible; there appear to be differential trends in some outcomes between urban and rural areas during our period of study. Nevertheless, the headline results are very similar with all LAs included. In the larger sample, we estimate slightly smaller drops in full-time education

[47] We also find that the EMA had no impact on GCSE point scores (the coefficient is –0.018 standard deviations, with a standard error of 0.018 and p-value of 0.326), or on the share of individuals in the different prior achievement groups.

participation of around two percentage points in Year 12 and 1.25 percentage points in Year 13. The estimated effects on qualifications remain limited, while earnings at age 17 still decline for high-attaining students. Effects on earnings in people's twenties remain negative, driven by high attainers. Effects on welfare benefit receipt are positive and, as in our matched sample, driven by low attainers. For crime, we continue to observe negative effects among the low prior attainment group. Our main conclusions are therefore not specific to the matched sample.

Fifth, we extend our analysis to all students in state schools in the matched areas, not just those eligible for free school meals. This provides evidence on whether the results are specific to the most disadvantaged subset of EMA-eligible students. FSM-eligible students account for roughly 15% of each cohort and we expect virtually all of them to be eligible for the full award of the EMA. Around 22% of students live in households with incomes low enough to qualify for the full EMA award but not low enough to receive free school meals, and a further 21% are eligible for a partial EMA award (although as discussed in Section I C, take-up rates were likely much lower among those eligible for partial awards). The estimates for the full sample are broadly consistent with our baseline findings, but are smaller in magnitude, which is expected as the estimates include the 42% of students who were ineligible for any EMA support and 21% who were only partially eligible. For instance, the overall participation effect at age 16-17 is 1.7 percentage points in the sample of all state-educated students, which is consistent with slightly larger effects for students eligible for the full award but not FSM.[48]

Sixth, we implement a placebo check in which we shift the treatment year to 2003, using 2002 as the final pre-treatment year and 2003 as the first post-treatment year. This reduces the sample to one cohort on either side of the reform. We find consistently small and statistically insignificant effects for almost all outcomes. Lastly, we perform an additional robustness check for criminal outcomes only. In addition to exploiting variation across cohorts and across areas, we add data on crimes committed before the age when individuals became eligible for the EMA for a triple-differences design. Again, the results are consistent with our main estimates.[49]

We report results across multiple outcomes and subgroups without formal adjustment for multiple hypothesis testing. Such adjustments would reinforce our main conclusion: the EMA did not improve labour market outcomes.

[48]The full-population effect is 0.017, while the free school meal population effect is 0.029. Assuming effects are half as large for the 21% of students eligible for the partial EMA award as for those eligible for the full award but not FSM (denoted x) and that private school students (around 7% of the population) are ineligible, the weighted average effect is $0.029 \times 0.15 + x \times 0.22 + 0.5 \times x \times 0.21 = (1 - 0.07) \times 0.017 \Rightarrow x = \frac{0.93 \times 0.017 - 0.029 \times 0.15}{0.325} = 0.035$.

[49]This is possible because—unlike our human capital outcomes—we observe criminal convictions from age 13 onward. The estimating equation is $y_{ijca} = \psi_i + \lambda^1_{ac} + \lambda^2_{aj} + \beta Post_c \times NR_j \times \mathbb{1}[a \geq 16] + \epsilon_{ijca}$ where $\mathbb{1}[a \geq 16]$ indicates that individuals have finished compulsory schooling, and β represents the parameter of interest. We include individual fixed effects ψ_i, which incorporate area-, cohort- and area-by-cohort fixed effects, as well as interactions of age and cohort and age and area in λ^1_{ac} and λ^2_{aj}. NR_j=1 in national roll-out areas. See Online Appendix Table SA2 for the estimates.

V B Impacts by gender, SEN status and ethnicity

We also explore heterogeneity in effects by gender, special educational needs (SEN) status, and ethnicity. These analyses are presented for a subset of education and labour market outcomes in Appendix Table A6. The results are broadly consistent with our main results for all subgroups: in all cases we observe increases in education participation (albeit smaller impacts for non-white students); reductions in training; reduced earnings at age 17 (although the smaller effects for girls and for SEN students); and no evidence of improved labour market outcomes up to age 31.[50]

VI Cost effectiveness

We assess the cost effectiveness of the EMA by calculating its Marginal Value of Public Funds (MVPF), the ratio of private benefits and net costs to the government. These calculations are shown in Table 9. The first column shows our central estimates, while the second column shows the lower and upper bounds given a 95% confidence interval, estimated through wild bootstrap simulations. All calculations assume zero effects beyond age 31.

Table 9: Costs and benefits of the EMA per FSM-eligible student

	Point Estimate	95% Confidence Interval
Costs to the Government		
Up-front payments	£2,290	[£2,220, £2,360]
Administration costs	£160	[£150, £160]
Additional education costs	£220	[£20, £420]
Reduction in taxes on income	£180	[-£90, £440]
Increased benefit payments	£340	[£100, £580]
Costs of crime processing	-£300	[-£660, £20]
Total	**£2,890**	**[£2,270, £3,470]**
Private benefits		
Up-front payments	£2,290	[£2,220, £2,360]
Changes to long-run post-tax earnings	-£480	[-£1,140, £230]
Increased benefit payments	£340	[£100, £580]
Reduction in crime victimhood	£300	[-£20, £640]
Total	**£2,450**	**[£1,710, £3,200]**
Marginal Value of Public Funds	0.85	[0.52, 1.29]

Notes: Additional education costs include the costs of additional years of full-time education—including the cost of providing additional free school meals—net of the savings from lower government support for training. Welfare benefit payments include additional child benefit paid to parents as a result of children remaining eligible for longer as well as out-of-work benefits to young adults. All values are in 2023/24 prices, discounted to age 16-17 (Year 12) values using a real discount rate of 3.5%. Values may not sum due to rounding. 95% confidence intervals constructed using the wild bootstrap method.

[50] All of the estimates for crime are small and statistically insignificant (we do not have the power to split the heterogeneity analysis by prior attainment). The results are available in Online Appendix Table SA3.

We estimate that the government spent approximately £2,290 on cash transfers per FSM-eligible young person (in 2023/24 prices). In addition, the EMA generated upfront administrative costs of £160 and costs for teaching the students who spent more time in education of £220. Based on our point estimates of the effect of the EMA on annual earnings, the EMA also led to long-run reductions in earnings and thus tax revenue of £180 and increases in welfare benefit claims of £340. These costs were only partially offset by a reduction of £300 in estimated costs to the criminal justice system from reduced offending.

Most of the private benefits of the programme were mechanical in the form of the direct payments to young people (£2,290). This is partially offset by our central estimate of a loss in long-run post-tax earnings of £480 per individual.[51] Conversely, increases in welfare benefit payments amounted to additional private benefits of £340, while reductions in crime and hence lower crime victimhood generated estimated private benefits of £300 per individual.[52]

We estimate the MVPF for the EMA was 0.85, implying that for every pound of government spending, recipients received only 85 pence in private benefits. The 95% confidence interval ranges from 0.52 to 1.29, with the MVPF exceeding 1 in only 24% of our bootstrap simulations. 1 is a natural benchmark, as it is the MVPF of a simple nondistortionary transfer: if the only effect of the programme were to move money from the government to recipients, costs and benefits would be equal and the MVPF would be exactly 1. Our results compare with average MVPFs of 0.74 (95% CI of $[0.36, 1.47]$) for cash transfers and ∞ (95% CI of $[17.83, \infty]$) for child education programmes reported in Hendren and Sprung-Keyser (2020).

VII Conclusion

This paper evaluates a large conditional cash transfer programme in the UK, exploiting its staggered roll-out across local areas in England. Despite the substantial value of the transfer, we find only modest effects on education participation and no overall improvement in labour market outcomes by age 31, implying that the EMA did not measurably improve upward social mobility for children from the poorest families. These findings may reflect the relatively generous support that was already available for low-income students in post-16 education in England. Education was free, travel was often subsidised, and families were eligible for various forms of financial support for children remaining in education, including Child Benefit and Child Tax Credit. This may help explain why similar cash transfer programmes in low- and middle-income countries—where such support is often absent—typically yield larger effects on participation and attainment.

The effects of the programme on labour market trajectories varied substantially depending on students' attainment at age 16. High attainers at age 16 improved their age 18 attainment and

[51]We ignore lower earnings at age 16-19 (the period when the EMA was operational). In our judgment, these reductions largely represent students choosing to reduce their part-time hours due to the income effect of the transfer rather than welfare losses. In contrast, reduced earnings in later years more likely reflect lower productivity, possibly due to lost early work experience.

[52]Further details on these costs and benefits can be found in Section 2 of the Online Appendix.

were more likely to attend university. In contrast, low attainers' education outcomes did not improve measurably even though they remained in education for longer, and they spent more time not in employment, education or training (NEET) in their late teens. Overall, the EMA's impact on labour market trajectories appears to have been neutral. Effects on crime were favourable but small relative to the programme's substantial cost. Our MVPF estimates suggest that the substantial resources devoted to the EMA generated little value beyond its direct transfer component, a disappointing conclusion for a programme explicitly designed to improve the life chances of the poorest young people.

Our findings offer two broader lessons for education policy. First, conditional cash transfers may be less effective in high-income settings where existing financial support is already substantial. Second, extending time in education by itself may do little to improve long-run outcomes—and may even displace beneficial training and early labour market experience.

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Appendix

A Sample selection

Our raw sample consists of all students who appear in the School Census of the National Pupil Database as being in Year 11 (the final year of compulsory education, and the year in which almost all students take their GCSE exams) between the 2001/02 academic year and the 2010/11 academic year (2008/09 for the crime results). This includes all children enrolled in state schools in England, which is approximately 93% of all students. Almost all FSM-eligible students are enrolled in state schools rather than (fee-paying) private schools. In total, we observe 550,000 to 600,000 individuals per cohort in the NPD.

To clean the data, we remove students who did not turn 16 in year 11 (e.g. because they skipped an academic year, which happens sometimes but rarely) and drop duplicate observations. We also remove those for whom we do not have information on their home Local Authority, their eligibility for Free School Meals, or their results in age 16 (GCSE) exams. We also remove students who lived in three Local Authorities: Suffolk, Sunderland and Lancashire. Each of these areas received non-standard versions of the EMA between 2000 and 2004, which gave students free bus travel instead of a weekly stipend. This leaves us with between 500,000 and 550,000 students in the cleaned sample in most cohorts, shown in column (2) of Table A1.

Table A1: Sample Selection

Academic year	(1) Raw sample	(2) Cleaned sample	(3) Cleaned sample (*Pilot* + *Matched* areas)	(4) FSM sample	(5) FSM sample (*Pilot* + *Matched* areas)	(6) FSM sample (*Pilot* + *Matched* areas with tax data)
2001/02	548,787	489,682	160,760	68,067	24,503	22,326
2002/03	572,228	537,969	175,534	75,665	27,240	24,881
2003/04	589,846	554,389	179,718	77,633	27,802	25,792
2004/05	585,918	551,515	177,458	76,244	27,206	25,463
2005/06	595,281	561,527	180,382	74,615	26,546	25,113
2006/07	602,163	568,130	184,113	73,571	26,542	25,298
2007/08	599,147	565,766	182,516	71,593	25,481	24,527
2008/09	580,320	547,822	175,115	71,023	25,324	24,599
2009/10	580,089	547,493	172,830	73,693	26,080	25,471
2010/11	569,012	537,297	169,242	75,645	26,232	25,717

Notes: Count of individuals in the LEO data. *Academic year* refers to the last compulsory academic year (Year 11). *Cleaned sample* includes both FSM and non-FSM students. *Pilot* + *Matched* refers to LAs included in the main analysis (which includes pilot areas). FSM stands for eligible to Free School Meals in Year 11.

Column (3) shows the number of students in the Local Authorities we use for our main analysis sample (the *Pilot* + *Matched* areas; see Section III B). Column (4) shows the number of students on Free School Meals in each cohort across all Local Authorities. Column (5) shows the number of students in our main analysis sample, which consists of FSM-eligible students in *Pilot* + *Matched* areas. There are around 25,000 students in our main analysis sample per cohort.

Records for a small subset of these students cannot be linked to tax data, and are therefore not used in estimates of the effect of the EMA on employment, welfare benefit receipt or earnings. Column (6) shows the number of individuals in the main analysis sample whose school records can be linked to their tax records. The match rate improved across cohorts, from 91% for students who finished compulsory schooling in 2002 to 98% for those who finished in 2011. This suggests that most of the missing observations can be explained by a failure to match school and tax records rather than because students never had any earnings that were reported to the tax authority. We therefore drop these individuals from the sample when looking at the effects of the EMA on earnings, though our results are not sensitive to this decision (see Online Appendix Table SA4).

B Nearest neighbour matching

The attributes at the LA level on which we match are: the Black share of the population of 15-16 year old students in the 2003/04 academic year, Asian share of the same population, the share that is not White, Black, or Asian, the share with a first language other than English, the share eligible for free school meals, the share living in urban areas (as defined by the Office for National Statistics in the 2001 Census), the mean overall score in age 11 standardised tests (standardised), the mean overall GCSE point score (standardised), the share achieving 5 good GCSE passes, and the mean Income Deprivation Affecting Children Index (IDACI). We also take from the 2001 census, across the whole population of the LA, the share of individuals who stayed in education past age 16, the share of the population in higher managerial occupations, the share in lower managerial occupations, the share in routine occupations, the share long-term unemployed, the share in owner-occupied housing, the share in social housing, the share with no qualifications, and the share with degree-level qualifications.

Areas are then matched using the construction of a propensity score for treatment into the early rollout areas. We then use one-to-one nearest-neighbour propensity score matching without replacement, using a caliper of 0.8 on the estimated propensity score. The nine pilot and nine pilot control areas are excluded from the matching procedure, as they were selected separately to the process for choosing early rollout areas. The full list of areas is shown in the Online Appendix Table SA1.

C Additional tables

Table A2: EMA take-up relative to number of students

School Year	2004/05	2005/06	2006/07	2007/08	2008/09
EMA recipients	297	430	527	546	576
Total student numbers	767	1,118	1,267	1,311	1,379
% of all students claiming EMA	39%	38%	42%	42%	42%

Notes: Descriptive statistics on the proportion of pupils eligible and claiming the EMA. Numbers expressed in thousands. *Source for recipients*: House of Commons Library (Bolton, 2011). Total student numbers reflect the number of students who would be eligible for the EMA in each academic year by cohort of birth, home address, and education status, but not necessarily parental income. Data taken from the NPD and then scaled to match the overall population. Pre-scaled data taken from data on post-16 education participation in the NPD and ILR for students in the cleaned sample of the School Census (see Table A1). Data scaled to match official statistics from the Department for Education on participation in full-time education in 2006/2007, the first year in which there was no difference across areas in eligibility. Scaling is used to adjust for the fact that the NPD does not capture students outside of state schools.

Table A3: Robustness: sample selection and controls, educational outcomes

	(1) No GCSE controls	(2) Exc. variants	(3) Exc. 2004	(4) All areas	(5) All students	(6) Placebo 2003
a. FT Ed.,	2.26**	3.10***	3.48***	2.05***	1.68***	0.62
age 16-17	(0.96)	(1.14)	(1.06)	(0.52)	(0.59)	(0.94)
	50.97	*50.97*	*50.97*	*53.44*	*65.81*	*51.11*
b. Training,	-1.08	-2.18***	-1.63*	-0.81**	-0.78**	-0.28
age 16-17	(0.69)	(0.80)	(0.82)	(0.37)	(0.33)	(0.74)
	18.29	*18.29*	*18.29*	*14.93*	*14.52*	*17.43*
c. NET,	-0.64	-0.09	-0.81	-1.15***	-0.64**	-1.64*
age 16-17	(0.77)	(0.71)	(0.77)	(0.42)	(0.30)	(0.97)
	24.25	*24.25*	*24.25*	*26.03*	*14.91*	*25.09*
d. FT Ed.,	1.14	1.15	1.87**	1.25***	1.31***	1.61*
age 17-18	(0.76)	(0.77)	(0.76)	(0.48)	(0.37)	(0.86)
	38.01	*38.01*	*38.01*	*39.44*	*53.65*	*37.39*
e. Training,	-1.43**	-1.75**	-1.85**	-1.06***	-0.90***	0.37
age 17-18	(0.62)	(0.74)	(0.73)	(0.32)	(0.26)	(0.65)
	18.67	*18.67*	*18.67*	*15.87*	*16.77*	*19.14*
f. NET,	0.90	1.21*	0.35	0.14	-0.04	-0.87
age 17-18	(0.77)	(0.63)	(0.67)	(0.44)	(0.29)	(1.00)
	35.38	*35.38*	*35.38*	*37.41*	*23.25*	*35.91*
g. Passing acad,	0.20	0.17	0.31	1.10***	0.89**	-0.42
(all)	(0.52)	(0.57)	(0.57)	(0.31)	(0.41)	(0.49)
	11.87	*11.87*	*11.87*	*13.06*	*31.45*	*12.05*
h. Passing acad,	1.75	2.30	2.93*	3.19***	1.90**	0.32
(high att.)	(1.56)	(1.48)	(1.52)	(0.82)	(0.76)	(1.63)
	46.01	*46.01*	*46.01*	*47.04*	*60.99*	*46.33*
i. Uni. enrol,	0.18	0.17	-0.07	-0.24	0.61**	0.26
(all)	(0.38)	(0.42)	(0.49)	(0.33)	(0.28)	(0.50)
	14.99	*14.99*	*14.99*	*15.87*	*33.05*	*14.74*
j. Uni. enrol,	2.62**	3.00*	1.91	1.79**	1.44**	1.21
(high att.)	(1.18)	(1.57)	(1.36)	(0.71)	(0.59)	(1.56)
	47.63	*47.63*	*47.63*	*47.45*	*60.21*	*47.15*
Observations *(a.-g.)*	108,660	81,640	107,375	302,725	699,020	53,530
Observations *(h.)*	25,065	18,900	25,960	79,075	340,785	11,430
Observations *(i.)*	108,660	81,645	107,380	302,730	699,030	53,535
Observations *(j.)*	25,065	18,900	25,960	79,075	340,785	11,430
Number of clusters	50	38	50	147	50	50

Notes: *No GCSE controls* excludes controls for GCSE attainment, *Exc. variants* omits areas where the EMA differed from the rest of the country (Bolton, Oldham, Doncaster, Nottingham, Gateshead, Stoke-on-Trent, Coventry, Leicester, Tameside, Wakefield, South Tyneside and Wigan). *Exc. 2004* excludes the first cohort where all individuals received the EMA and replaces it with the 2006 cohort. Column 5 includes all pupils irrespective of eligibility for FSM. *Placebo 2003* is a placebo test where the 2003 cohort is included as having received the EMA. All values are multiplied by 100 so they can be interpreted as percentage point changes. Year 12 is the academic year in which nearly all students turn 17. Standard errors clustered at the LA level are shown in parentheses. The mean of the dependent variable across individuals in the two pre-roll-out cohorts in the national roll-out LAs is given in italics. *,** and *** indicate significance at the 90%, 95%, and 99% confidence level.

Table A4: Robustness: sample selection and controls, labour market outcomes

	(1) Fewer controls	(2) Exc. variants	(3) Exc. 2004	(4) All areas	(5) All students	(6) Placebo 2003
a. Earnings,	-151***	-90	-145**	-54	-111**	50
age 17 (all)	(54)	(64)	(72)	(39)	(48)	(82)
	2,168	*2,168*	*2,168*	*2,410*	*2,751*	*1,935*
b. Earnings,	-377***	-295**	-421***	-224***	-172***	-27
age 17, (high att.)	(109)	(123)	(118)	(74)	(55)	(125)
	2,522	*2,522*	*2,522*	*2,668*	*2,550*	*2,272*
c. Earnings in FT Ed.,	-129**	-147**	-155***	-112***	-103***	63
age 17 (all)	(49)	(55)	(54)	(35)	(38)	(98)
	1,322	*1,322*	*1,322*	*1,556*	*1,754*	*1,139*
d. Earnings in FT Ed.,	-286***	-292**	-295***	-190***	-139***	1
age 17, (high att.)	(86)	(116)	(78)	(54)	(41)	(135)
	1,683	*1,683*	*1,683*	*1,881*	*1,812*	*1,502*
e. Log earnings,	-0.69	-0.32	-0.59	-0.52	0.07	-0.23
age 20-31 (all)	(0.53)	(0.65)	(0.56)	(0.33)	(0.24)	(0.93)
	19,187	*19,187*	*19,187*	*20,080*	*22,709*	*19,299*
f. Log earnings,	-1.30	0.87	-1.28	-1.75*	-0.77	-2.07
age 20-31 (low att.)	(1.39)	(1.76)	(1.54)	(0.94)	(1.02)	(2.05)
	15,842	*15,842*	*15,842*	*16,367*	*16,571*	*16,052*
g. Log earnings,	-1.15	-1.12	-1.29	-0.93*	0.14	0.95
age 20-31 (high att.)	(0.89)	(1.04)	(1.01)	(0.54)	(0.34)	(1.63)
	22,375	*22,375*	*22,375*	*23,539*	*25,192*	*22,514*
h. Out of work benefit,	0.74	0.61	0.58	0.18	-0.03	0.36
age 20-31 (all)	(0.45)	(0.58)	(0.52)	(0.26)	(0.17)	(0.51)
	25.36	*25.36*	*25.36*	*22.68*	*11.39*	*24.54*
i. Out of work benefit,	1.80*	2.16**	0.79	1.40**	1.52**	0.62
age 20-31 (low att.)	(0.90)	(1.06)	(0.87)	(0.56)	(0.68)	(1.11)
	44.45	*44.45*	*44.45*	*41.06*	*38.25*	*42.97*
j. Out of work benefit,	-0.15	-0.62	-0.05	-0.28	-0.00	-0.32
age 20-31 (high att.)	(0.49)	(0.61)	(0.54)	(0.33)	(0.12)	(0.61)
	8.61	*8.61*	*8.61*	*7.61*	*3.20*	*8.25*
Observations *(a.)*	100,105	75,195	99,415	276,825	660,675	48,760
Observations *(b.)*	23,435	17,705	24,395	73,505	326,305	10,555
Observations *(c.)*	42,685	32,410	43,655	129,420	373,925	19,500
Observations *(d.)*	17,390	13,170	18,075	56,450	259,550	7,700
Observations *(e.)*	569,620	431,445	552,545	1,606,135	4,966,570	277,680
Observations *(f.)*	81,695	61,560	76,930	211,090	264,425	38,685
Observations *(g.)*	183,615	139,105	185,215	566,105	2,817,735	83,200
Observations *(h.)*	1,303,910	979,715	1,261,935	3,632,785	8,388,360	642,395
Observations *(i.)*	314,375	232,610	302,520	798,205	856,355	146,015
Observations *(j.)*	300,805	226,810	304,065	948,910	4,089,395	137,135
No. of clusters	50	38	50	147	50	50

Notes: See notes to Table A3.

Table A5: Robustness: sample selection, crime outcomes

	(1) No GCSE controls	(2) Exc. variants	(3) Exc. 2004	(4) All areas	(5) All students	(6) Placebo 2003
a. Conviction,	0.02	0.04	-0.14	-0.14	-0.04	-0.13
age 16-18 (all)	(0.26)	(0.30)	(0.25)	(0.13)	(0.07)	(0.29)
	4.76	*4.47*	*4.47*	*4.01*	*2.08*	*4.18*
b. Conviction,	-0.82	-0.28	-0.84	-0.58	-0.01	0.08
age 16-18 (low att.)	(0.71)	(0.79)	(0.64)	(0.40)	(0.40)	(0.81)
	11.38	*11.38*	*11.38*	*10.15*	*10.18*	*10.95*
c. Conviction,	0.39*	0.37	0.22	0.06	0.01	-0.29
age 16-18 (med att.)	(0.24)	(0.30)	(0.25)	(0.14)	(0.11)	(0.34)
	3.04	*3.04*	*3.04*	*3.00*	*2.28*	*3.18*
d. Conviction,	-0.28*	-0.21	-0.25	-0.05	-0.04	-0.34
age 16-18 (high att.)	(0.17)	(0.16)	(0.18)	(0.09)	(0.03)	(0.25)
	0.83	*0.83*	*0.83*	*0.73*	*0.41*	*0.92*
e. Conviction,	-0.16	-0.25*	-0.22	-0.20**	-0.04	0.03
age 19-29 (all)	(0.13)	(0.13)	(0.15)	(0.08)	(0.04)	(0.18)
	3.88	*3.74*	*3.74*	*3.38*	*1.83*	*3.52*
f. Conviction,	-0.91**	-0.84**	-0.94**	-0.44*	-0.17	0.07
age 19-29 (low att.)	(0.34)	(0.36)	(0.43)	(0.23)	(0.19)	(0.52)
	8.16	*8.16*	*8.16*	*7.27*	*7.11*	*7.99*
g. Conviction,	0.04	-0.00	-0.08	-0.13	-0.02	-0.08
age 19-29 (med att.)	(0.14)	(0.16)	(0.15)	(0.09)	(0.06)	(0.19)
	3.03	*3.03*	*3.03*	*2.95*	*2.27*	*3.05*
h. Conviction,	-0.12	-0.16*	-0.04	-0.08	-0.02	0.02
age 19-29 (high att.)	(0.09)	(0.09)	(0.11)	(0.05)	(0.02)	(0.17)
	0.90	*0.90*	*0.90*	*0.81*	*0.48*	*0.87*
Observations *(a.)*	217,326	160,404	211,058	595,234	1,387,008	103,488
Observations *(b.)*	52,394	38,768	51,574	132,996	142,732	24,336
Observations *(c.)*	110,980	83,834	107,554	304,068	562,672	56,296
Observations *(d.)*	50,136	37,802	51,930	158,164	681,604	22,856
Observations *(e.)*	1,086,630	802,020	1,028,719	2,976,170	6,935,040	517,440
Observations *(f.)*	261,970	193,840	251,147	664,980	713,660	121,680
Observations *(g.)*	554,900	419,170	525,369	1,520,340	2,813,360	281,480
Observations *(h.)*	250,680	189,010	252,203	790,820	3,408,020	114,280
Number of clusters	50	38	50	147	50	50

Notes: Column definitions are as in Table A3. All values are multiplied by 100 so they can be interpreted as percentage point changes. Standard errors clustered at the LA level are shown in parentheses. The mean of the dependent variable (indicating the average rate of convictions in a given year) is given in italics. *,** and *** indicate significance at the 90%, 95%, and 99% confidence level.

Table A6: Effect of the EMA on education and labour market outcomes by student characteristics

	(1) Boys	(2) Girls	(3) SEN	(4) No SEN	(5) White	(6) Non-white
a. Full-time education,	3.45***	2.32**	4.36***	2.23**	3.32***	1.24
age 16-17	(1.07)	(1.08)	(1.40)	(0.95)	(1.05)	(0.94)
	46.59	*55.40*	*38.91*	*56.30*	*47.21*	*67.41*
b. Training,	-1.75*	-0.67	-1.59	-0.98	-1.37	-0.74
age 16-17	(0.89)	(0.87)	(1.19)	(0.72)	(0.90)	(0.94)
	20.44	*16.12*	*21.94*	*16.68*	*20.02*	*10.71*
c. University enrolment	0.83*	0.02	-0.49	0.84*	0.32	1.09
	(0.44)	(0.48)	(0.39)	(0.49)	(0.39)	(1.16)
	13.23	*16.77*	*5.36*	*19.25*	*10.55*	*34.41*
d. Earnings,	-215**	-91*	-74	-189***	-133**	-243**
age 17	(97)	(53)	(80)	(69)	(63)	(93)
	2,459	*1,859*	*1,738*	*2,358*	*2,303*	*1,553*
e. Log earnings,	-0.02	-1.33	0.41	-0.99	-0.51	-0.71
age 20-25	(0.66)	(0.87)	(1.15)	(0.71)	(0.66)	(1.21)
	18,304	*15,945*	*15,046*	*17,924*	*17,228*	*16,951*
f. Log earnings,	-0.05	-1.55	0.25	-1.06	-0.95	0.45
age 26-31	(0.75)	(1.00)	(1.36)	(0.72)	(0.69)	(1.47)
	23,499	*18,237*	*18,663*	*21,794*	*20,814*	*21,895*
Observations *(a.-c.)*	54,580	54,075	33,005	75,655	85,115	23,545
Observations *(d.)*	51,145	48,960	30,380	69,725	78,915	21,190
Observations *(e.)*	137,350	116,210	59,020	194,540	201,215	52,340
Observations *(f.)*	175,880	140,180	71,580	244,480	244,960	71,100
No. of clusters	50	50	50	50	50	50

Notes: Sample consists of FSM-eligible young people who were resident in *Pilot+Matched* LAs and finished compulsory schooling between 2002 and 2005. All values except for those in £ are multiplied by 100 so they can be interpreted as percentages or percentage point changes. Monetary values expressed in 2023/24 prices. Standard errors clustered at the LA level are shown in parenthesis. The mean of the dependent variable across individuals in the two pre-roll-out cohorts in the national roll-out LAs is given in italics. *,** and *** indicate significance at the 90%, 95%, and 99% confidence level.